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The rôle of atmospheric disturbances in the general circulation

By E. PALMÉN
Academy of Finland

Symons Memorial Lecture, delivered 21 March 1951

1. Introduction

To begin with I want to express my gratitude to the President of the Royal Meteorological Society for the honour he has conferred on me by giving me opportunity to deliver the Symons Memorial Lecture. I appreciate thankfully this opportunity to present to the members of this Society some of the research carried out in collaboration with a group of meteorologists at the University of Chicago since 1946.

As the subject for my lecture to-day I have selected the problem of the rôle of atmospheric disturbances in the general circulation. This problem is one of the central questions in dynamic meteorology and in climatology. Whereas in earlier meteorology the general circulation was treated more or less as a separate problem, very little connected with atmospheric disturbances, the introduction of the polar front theory by the Norwegian school of meteorologists and the new ideas expressed by Jeffreys (1926) have brought atmospheric disturbances into the foreground as essential cells in the meridional exchange of air masses, heat and angular momentum.

Since the problem selected for this lecture has been the subject of a very large number of investigations it is not possible to give any complete references to previous papers in the same field. A very interesting survey of the present tendencies in the treatment of the problem can be found in the *Centenary Proceedings of the Royal Meteorological Society*, 1950. The contributions by Petterssen (1950) and Eady (1950) demonstrate particularly the complexity of the problem. It is probably no exaggeration to characterize the present development as 'a tendency to regard what is commonly called the general circulation as nothing but the statistical manifestation of currents as they appear in the actual atmosphere.' However, both climatological and synoptic evidence forces us to see something more than these irregular movements ; thus the mean basic current system, commonly called the general circulation, can still be defined as a scientific problem.

2. Formulae for meridional transport of angular momentum

The transfer of angular momentum $\left[M_\phi\right]_{z_1}^{z_2}$ per unit time through a vertical 'wall' around the hemisphere at latitude ϕ between the levels z_1 and z_2 is given by the expression

$$\left[M_\phi\right]_{z_1}^{z_2} = 2\pi a^2 \cos^2\phi \left\{ \int_{z_1}^{z_2} \omega a \cos\phi \, \overline{\rho v} \, dz + \int_{z_1}^{z_2} \overline{\rho u v} \, dz \right\} \quad . \quad (1)$$

Here a is the radius of the earth, ω is the angular velocity of the earth's rotation, ρ is the density, and u, v are the zonal and meridional components of the air movement. The bars indicate mean values around the parallel circle. Thus the momentum transfer across a latitude circle can be separated into two terms, the first representing the flux of angular momentum due to the rotation of the earth which we could call ' ω-momentum,' the second representing the transport of ' relative momentum ' or ' u-momentum.' In deriving Eq. (1) the small contribution from the variable height has been neglected.

The first right-hand term in Eq. (1) vanishes if there is no net mass transport in the layer between z_1 and z_2. If we select sufficiently long time periods and neglect seasonal variations we can regard the atmospheric mass distribution as constant. For this case

$$\int_0^\infty \overline{\rho v} \, dz = 0 \quad . \quad . \quad . \quad . \quad (2)$$

and consequently the flux of ω-momentum vanishes if the integration in Eq. (1) is extended from the earth's surface to the upper limit of the atmosphere. Hence the total transfer of angular momentum through a given latitude ϕ can be expressed by Jeffreys's (1926) formula :

$$\left[M_\phi\right]_0^\infty = 2\pi a^2 \cos^2\phi \int_0^\infty \overline{\rho u v} \, dz \quad . \quad . \quad . \quad . \quad (3)$$

or, if instead of level surfaces we use pressure p as the vertical variable, by :

$$\left[M_\phi\right]_{p_0}^{0} = \frac{2\pi a^2 \cos^2\phi}{g} \int_0^{p_0} \overline{uv} \, dp \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where p_0 denotes the pressure at the earth's surface and g is the gravity constant. In both cases we neglect the varying topography of the earth, and in (4) p_0 can be regarded as the mean surface pressure at latitude ϕ.

The net meridional transfer of angular momentum depends, according to the above formulae, upon the correlation between u and v. If $\bar{u}$, $\bar{v}$ denote the mean values of u and v at a given pressure surface p at latitude ϕ and u', v' are the local deviations from the mean values at the parallel circle, Eq. (4) can be written :

$$\left[M_\phi\right]_{p_0}^{0} = \frac{2\pi a^2 \cos^2\phi}{g} \left[\int_0^{p_0} \bar{u}\,\bar{v} \, dp + \int_0^{p_0} \overline{u'v'} \, dp \right] \quad . \quad . \quad (5)$$

The first term in the right-hand expression then gives the net momentum flux through latitude ϕ due to a meridional flow or ' drift ' of air at the different isobaric surfaces, whereas the second term has the formal character of a Reynolds stress. Following Priestley (1950) we shall call the first term the ' drift term ' and the second one the ' eddy flux term.'

3. The tropical circulation cells

The principal source region for angular momentum is the tropical and subtropical east-wind belt, and the principal sink regions are the extra-tropical belts of westerlies in both hemispheres. The angular momentum generated in the source

egion must be transported in some fashion to the northern and southern sink egions. The latitudes of the maximum poleward flux of angular momentum herefore coincide with the latitudes of the northern and southern subtropical igh-pressure belt, and since $\left[M_\phi\right]_{p_s}^{0}$ gives the total flux of momentum across any atitude this expression reaches its maximum at those latitudes.

The drift term in Eq. (5) depends upon the existence of one or several circu- ation cells in a meridional plane. Because the mean value of the geostrophic meri- ional wind for any complete latitude circle is zero the mean drift is equal to the nean value of the ageostrophic component $\overline{\Delta v}$. The drift term in Eq. (5) gives a ositive contribution if there is, in the vertical, a positive correlation between $\bar{u}$ nd $\overline{\Delta v}$. Thus the vertical circulation can contribute positively to a poleward ux of angular momentum only if the poleward drift occurs, on the average, in ayers with stronger west wind than does the equatorward drift. Consequently he transport of angular momentum is most effective if the meridional circulation onsists of two cells with a maximum poleward drift around the tropopause and n the upper troposphere and equatorward drifts in the lowest troposphere and in he upper stratosphere. Of these cells the tropospheric cell naturally is the most mportant ; only this cell will be considered here.

The poleward flux of momentum due to the drift term in Eq. (5) is proportional o the speed of the zonal wind. The latitude of maximum $\left[M_\phi\right]_{p_s}^{0}$ must therefore also pproximately coincide with the latitude of strongest increase of west wind with eight in the troposphere. Now the mean surface wind is always relatively weak. Consequently, if the momentum flux is primarily determined by the drift term, *he strongest west wind in the upper troposphere must be observed almost vertically bove the subtropical high-pressure belt, i.e. around latitude* 30°.

Computations of the mean zonal wind in several meridional cross sections how that the belts of strongest west wind in both hemispheres really are situated lmost vertically above the subtropical high-pressure belts. I refer for the southern emisphere to the meridional cross sections for the meridians through Australia nd New Zealand, recently published by Loewe and Radok (1950) and by Hutchings 1950), where the belt of strongest westerlies has, especially during the southern inter, the character of a concentrated ' jetstream ' with mean velocities around 0 m sec^{-1} or more just north of latitude 30°S. From cross sections computed by Villett (1944), Hess (1948), and Yeh (1950), the two first mentioned in the region f North America, the third in the region of China, the same relationship between ne upper jetstream and the surface high-pressure belt is found in the northern emisphere. I refer later to the paper by Namias and Clapp (1949) who have found nat the upper jetstream in the northern hemisphere in January appears as a con- nuous stream around the whole hemisphere, the mean position of this hemispheric et being latitude 25°N.

In order to avoid misunderstanding it should be pointed out that this upper et computed from mean meridional cross sections for seasons or longer periods not identical with the ' meandering ' jetstream associated closely with polar front isturbances. This latter phenomenon can hardly be studied by the aid of climato-

logical data, as Palmén and Newton (1948) have shown, because of the strong irregular north-south displacements of the polar front and the principal air masses. Both jet phenomena are probably closely connected with meridional circulations (Namias and Clapp 1949 ; Chaudhury 1950).

The existence of a toroidal meridional circulation in the tropical and subtropical zones can hardly be doubted. Riehl and Yeh (1950) have computed the equatorward mass transport from surface observations in the zone between 40°N. and 40°S. They found an average equatorward flow of 50 to 250 cm sec^{-1} between 40°N. and the equator in January with the maximum value around latitude 13°N. Corresponding values were computed for the southern hemisphere in July. During the summer seasons the equatorward air flow was somewhat less pronounced, but still existed in a broad zone.

Using the very conservative assumption that this equatorward air transport is limited entirely to a surface layer with a total thickness of one km Riehl and Yeh computed the mean return flow between 1 and 13 km to be between 4 and 18 cm sec^{-1}. It is probable that the actual equatorward flow comprises a considerably deeper layer and that consequently the return flow in the upper troposphere is much stronger than the values given by Riehl and Yeh. The authors therefore regard their results as minimum values. There are no observational data indicating that the average return flow in the upper troposphere between latitudes 10° to 30° could not reach values of 50 cm sec^{-1} or even more.

It is easy to see that a poleward upper flow of about 50 cm sec^{-1} is far from sufficient to transport the total momentum generated in the zone of easterlies across the latitudes 30°. For the southern hemisphere Priestley (1950) has computed the generation of angular momentum in the zone of surface easterlies to be $1{\cdot}3 \times 10^{34}$ g cm^2 sec^{-1} per year, whereas computations made by Widger (1949) for the belt between 30° and 10°N. indicate a still stronger generation. Priestley concludes from his data that the upper poleward drift should be of the order of magnitude 300 cm sec^{-1} in order to transport the total angular momentum generated in the belt of easterlies into the extratropical zones of westerlies.

We are therefore probably forced to assume that the second term in Eq. (5), the eddy-flux term, is the more important. The term depends essentially upon large-scale disturbances, which explains why, as Priestley points out, the classical theory that zonal motions are maintained largely by circulations in meridional planes does not enjoy current favour. However, as we shall see later on, *the circumstance that the eddy term in the expression for the meridional transfer of momentum seems to be more important than the drift term does not mean that meridional circulations are of minor importance for a full understanding of the whole mechanism of the general circulation.*

Before we can go over to a discussion of this mechanism, let us first study the eddy term in Eq. (5). In discussing this term we are dealing with some kind of super-large-scale eddy motion where cyclones and anticyclones, long upper waves and other disturbances of similar dimensions are the eddies concerned. Because the real wind in such disturbances can be replaced, mostly with great accuracy, by the geostrophic wind the total momentum flux across a zonal ' wall ' at latitude ϕ can be approximately determined by the formula

$$\left[M_\phi\right]_{p_s}^{0} \simeq \frac{2\pi a^2 \cos^2\phi}{g} \int_0^{p_s} \overline{u_g v_g}\, dp \quad . \quad . \quad . \quad . \quad (6)$$

here u_g, v_g now are the geostrophic components of the zonal and meridional winds. ecause $\bar{v}_g$ according to definition is zero the geostrophic approximation also means at any net drift of air in a given isobaric layer is neglected. Using the geostrophic ssumption we also therefore exclude according to Eq. (1) all transfer of ω-momentum.

Recent investigations carried out in a very systematic way on a hemispherical cale in Massachusetts Institute of Technology and at the University of California n Los Angeles have shown that Eq. (6) gives rather satisfactory values for the total ux of angular momentum through different latitudes. According to a report resented by Mintz (1951) the geostrophic-momentum transfer for all latitudes etween 20° and 75°N. has been computed for January 1949 using daily charts for ea level, 700, 500, 300 and 200 mb over the whole northern hemisphere, and 100 mb harts over three quarters of the hemisphere. A net poleward flux was found in all titudes, with a maximum flux of 6×10^{26} g cm^2 sec^{-2}, near latitude 33°N. This titude for the maximum flux is thus in close agreement with the principle that the naximum flux of momentum should be around the latitude of highest mean surface ressure.

The amount 6×10^{26} is larger than the average yearly generation of angular nomentum between 0° and 30°S. computed by Priestley to be $4{\cdot}1 \times 10^{26}$ g cm^2 sec^{-2}, ut smaller than the amount computed by Widger for the belt 10° to 30°N. for nuary 1946 which was $7{\cdot}6 \times 10^{26}$ g cm^2 sec^{-2}. Remembering that Widger omitted he whole belt between 0° and 10°N., the total flux across latitude 30°N. should e considerably greater than that computed by Mintz. However, the determination f the generation of angular momentum in the zone of easterlies is necessarily rude. It is therefore impossible to decide whether the use of geostrophic winds ives satisfactory values or whether one could, from the difference between the eneration of angular momentum and meridional transfer computed from geostrophic inds, determine the transfer arising from the ageostrophic wind. Moreover Vidger's and Mintz's values are computed for different periods, and, we must cognise the possibility of a systematic transport of momentum from the northern the southern hemisphere. For comparison it may be mentioned that an average oleward drift of 50 cm sec^{-1} between the 600 and 100 mb surfaces would, con-dering the mean distribution of zonal wind, give a momentum transport of about 2×10^{26} g cm^2 sec^{-1}, which is about 20 per cent of the value computed by Mintz.

In order to arrive at a more satisfactory idea of the dynamic processes involved e must return to the complete Eq. (1) and compare the first term, the transfer ω-momentum, with the second, which depends mainly on the effect of large-ale eddies. Let us for this purpose consider rings of the atmosphere bounded by uasi-horizontal pressure surfaces at 600 and 200 mb and by vertical walls at latitudes °, 15°, 20°, 25° and 30°N. Let us further assume that the mean northward drift $\bar{v}$ in this layer is such that no mass moves through the pressure surfaces and that erefore

$$\overline{\Delta v} \cos\phi = \text{constant} \quad . \quad . \quad . \quad . \quad (7)$$

he flux of ω-momentum through the latitude circles in the layer 400 mb deep and

the corresponding differences determining the generation of relative angular momentum in the rings per unit value of $\overline{\Delta v} \cos \phi$ are given by the following table :

TABLE I

Latitude	Flux of ω-momentum g cm^2 sec^{-2}	Generation of relative momentum
10°	$4{\cdot}68 \times 10^{25}$	
		$0{\cdot}18 \times 10^{25}$
15°	$4{\cdot}50 \times 10^{25}$	
		$0{\cdot}23 \times 10^{25}$
20°	$4{\cdot}27 \times 10^{25}$	
		$0{\cdot}29 \times 11^{25}$
25°	$3{\cdot}96 \times 10^{25}$	
		$0{\cdot}34 \times 10^{25}$
30°	$3{\cdot}62 \times 10^{25}$	

The angular momentum generated in the rings appears as relative angular momentum. Since, according to our assumption, the zonal motion does not change with time the relative momentum generated as a result of the drift must be transported away either in a horizontal or in a vertical direction. If the equatorward drift in the isobaric layer 1000 to 600 mb is equally strong, as it must be if the meridional circulation is restricted to the levels between 1000 and 200 mb, an equal amount of angular momentum is lost in the lower latitudinal rings. The angular momentum lost in these lower rings can then be replaced by transport from the earth due to surface friction and mountain effects and by eddy exchange with the layer above. For this vertical eddy flux we thus get the right direction, *viz.* from higher relative momentum to lower momentum, contrary to the results presented by Widger who was forced to introduce a hypothetical upward flux (Widger, 1949 ; Fig. 3).

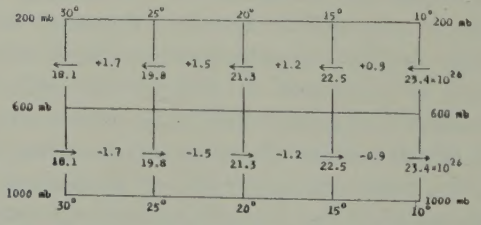

Figure 1. Net flux of ω-momentum across different latitude circles in the layers 1000-600 mb and 600-200 mb for a mean poleward drift $\overline{\Delta v} \cos \phi = 50$ cm/sec. The figures in the different blocks give the relative angular momentum (u-momentum) in units of 10^{25} g cm^2 sec^{-2} generated in each block.

In Fig. 1 quantitative values of the meridional flux of ω-momentum and corresponding values of generation or loss of relative momentum are given for a mean drift $\overline{\Delta v} \cos \phi = 50$ cm sec^{-1}. The sum of relative momentum generated per unit time in the rings between latitude 10° and 30°N. and between 600 and 200 mb is

·2 × 10^{26} g cm^2 sec^{-2}. A part of this angular momentum is transported downward s a result of the vertical eddy flux, but a larger part must be transported northward wing mainly to the large-scale eddy flux expressed by the term $\int_{p_2}^{p_1} \overline{u'v'}\, dp$ or approx-mately by $\int_{p_2}^{p_1} \overline{u_g v_g}\, dp$. The generation of relative angular momentum in the layer 00 to 200 mb is then, assuming an average drift of 50 cm sec^{-1}, of the same order of nagnitude as the eddy flux of angular momentum across latitude 30°N. computed y Mintz. It might be pointed out that in addition to the values in Table I there is lso to be considered the much smaller transport of relative momentum proportional o $\int_{p_2}^{p_1} \bar{u}\, \overline{\Delta v}\, dp$ which has been neglected here.

Concluding this part of the discussion we can summarize the results in the following ay : the angular momentum transferred from the earth to the atmosphere in the ropical source region is transported poleward largely because of the ' transport apacity ' of large-scale disturbances. To make this ' eddy transport ' possible it s, however, necessary to transform a part of the angular momentum of the absolute ovement, ω-momentum, into relative momentum, u-momentum. For that purpose mean toroidal circulation with poleward drift in the upper troposphere and equator-ard drift in the lower troposphere is necessary. This circulation derives its energy rom the distribution of heat sources and sinks in the atmosphere.

4. Mechanism of the eddy transfer of angular momentum

The eddy transfer of angular momentum from the tropical source region to he extratropical sink region is directed from higher to lower values of absolute ngular momentum, since the mean horizontal shear of the zonal wind $\partial u/\partial\phi$ satisfies he inequality :

$$\frac{\partial u}{\partial \phi} < 2\omega a \sin\phi + u \tan\phi \qquad . \qquad . \qquad . \qquad (8)$$

At low latitudes however, south of the subtropical jetstream, the eddy transport f angular momentum, according to values presented by Widger and Mintz, is irected from lower to higher values of relative momentum. It is therefore not ossible to express Eq. (6) by use of an *Austausch* coefficient, because this coefficient ould then be negative.

In order to get an idea of the mechanism involved it is convenient to compare he eddy transfer of momentum with the vertical flux of heat across a quasi-horizontal ressure surface. In a stable atmosphere the turbulent flux of heat is directed ownward in accordance with the gradient of potential temperature. We then neg-ect the other type of turbulent heat flux depending upon certain types of temperature uctuations in the isobaric surface (Robinson 1951).

The downward eddy flux of heat in a stable atmosphere depends upon the fact hat downward-moving air parcels have higher potential temperature, and thus so higher absolute temperature, than have upward-moving parcels when passing hrough the pressure surface. The heat flux is then proportional to $\overline{wT}$; w and T re negatively correlated in a stable atmosphere, and consequently the heat flux is irected downwards.

This analogy could be used for a better understanding of the meridional eddy flux of momentum. Starting from an arbitrary situation with purely zonal motion and a corresponding axially symmetric pressure field we can superimpose upon this motion certain disturbances in which the wind components are, to begin with, ageostrophic. Where the air passes across a given parallel from lower to higher latitude the u-component must then have a higher value than where the air moves to lower latitudes. That follows from the equation for horizontal motion in the form

$$\frac{du}{dt} = 2\omega \sin \phi \, \Delta v \quad . \quad . \quad . \quad . \quad (9)$$

The disturbed motion, however, very soon influences the pressure distribution. Hence the ageostrophic meridional wind component Δv is in most real cases small compared with the geostrophic component v_g.

White (1951) has recently computed the meridional eddy transport of heat from geostrophic and observed winds over North America. He found that the transport computed from the observed winds was, on the average, about 20 per cent larger than the geostrophic transport. Because the eddy transport of heat is proportional to $\overline{Tv}$ we can write

$$\overline{Tv} = \overline{Tv_g} + \overline{T\Delta v} \quad . \quad . \quad . \quad (10)$$

Since $\overline{Tv} > \overline{Tv_g}$, Δv must, on the average, be positively correlated with v_g. Between a western trough and an eastern ridge the ageostrophic meridional component Δv must therefore be larger than between a western ridge and an eastern trough. It follows from Eq. 9 that there should be a positive correlation between u and v because the absolute angular momentum is partly conserved during the meridional displacement of the air parcels.

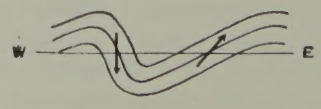

Figure 2. Characteristic shape of an upper ' tilted ' trough and probable distribution of ageostrophic wind.

Fig. 2 gives a schematic qualitative picture of a possible systematic deviation of the real wind from the geostrophic wind on both sides of an upper trough. Since the average west wind should be stronger on the east side than on the west side of the trough line and the pressure distribution tends to change according to the wind distribution one can also expect the pressure trough to get a tilted shape as in Fig. 2. Troughs of this tilted shape, however, necessarily also result in a geostrophic poleward flux of momentum (J. Bjerknes 1948 ; Starr 1948). It therefore seems natural that a computation of geostrophic momentum transfer should give results which approximately agree with the momentum flux computed from actual wind observations according to Eq. (4).

The author is aware of the weakness in the qualitative explanation presented here. It is, however, obvious that the maintenance of the relative atmospheric

motion with all its characteristics is impossible without systematic ageostrophic winds associated with certain types of circulations unequally distributed in the atmosphere. The discussion is intended to emphasize why the actual meridional eddy transfer of angular momentum can be computed approximately from the pressure field, as shown by Widger and Mintz.

5. Meridional circulation and mass exchange in extratropical regions

The processes associated with meridional transfer of angular momentum and exchange of air masses in lower latitudes can partly be discussed and investigated by the aid of mean data. That is, however, hardly possible in extratropical regions where the rapidly moving and changing disturbances are much more dominating. If there exist systematic circulations in meridional planes they must be to a much larger extent the statistical manifestation of very irregular and complicated movements. It is therefore easy to understand why most meteorologists regard these movements as so chaotic that they prefer statistical methods in dealing with them. The idea of large-scale turbulence, *Grossturbulenz*, as a substitute for real knowledge has therefore become rather popular. It seems to the author necessary to protest against this tendency. The conception of eddy transfer of certain properties was originally introduced in meteorology because it was the only possible way to consider small-scale turbulence ; it was necessary to create some ' order in disorder.' When dealing with large-scale disturbances such as cyclones, anticyclones and long waves other methods are, however, applicable.

With the rapid growth in the number of upper-air observations during the last 15 years it has become possible, for the first time in the history of meteorology, to extend three-dimensional synoptic studies to an almost hemispherical scale. Considering this progress it is rather surprising to find how little has really been done in this field. Very few synoptic studies have utilized the whole material available. In most cases superficial statistical methods or extensive use of data have replaced detailed investigations of individual cases which could have given much better information about the physical and dynamical processes involved.

The extratropical disturbances appear as quasi-horizontal vortices or wave-shaped perturbations where the vertical components of the air motion are suppressed to a very large extent. However, from the view-point of energy the vertical components are essential because they constitute the necessary links in all vertical circulations. The importance of the large-scale vertical motions in extratropical disturbances appears clearly from recent studies of vorticity changes and divergence (Sawyer 1949, Sheppard 1949) and from synoptic studies carried out at the University of Chicago (Hsieh 1950 ; Palmén and Newton 1951).

The life history of extratropical cyclones shows the characteristic features of an irreversible process where the warm air is lifted (occlusion) and the cold air sinks. This phenomenon is well known to all meteorologists in dealing with the lowest tropospheric layers. In the middle layers of the troposphere, *e.g.*, 500 mb, the cold air forms troughs whereas the warm air appears as ridges. The boundary between the two air masses is very often distinct up to about 400 mb and sometimes

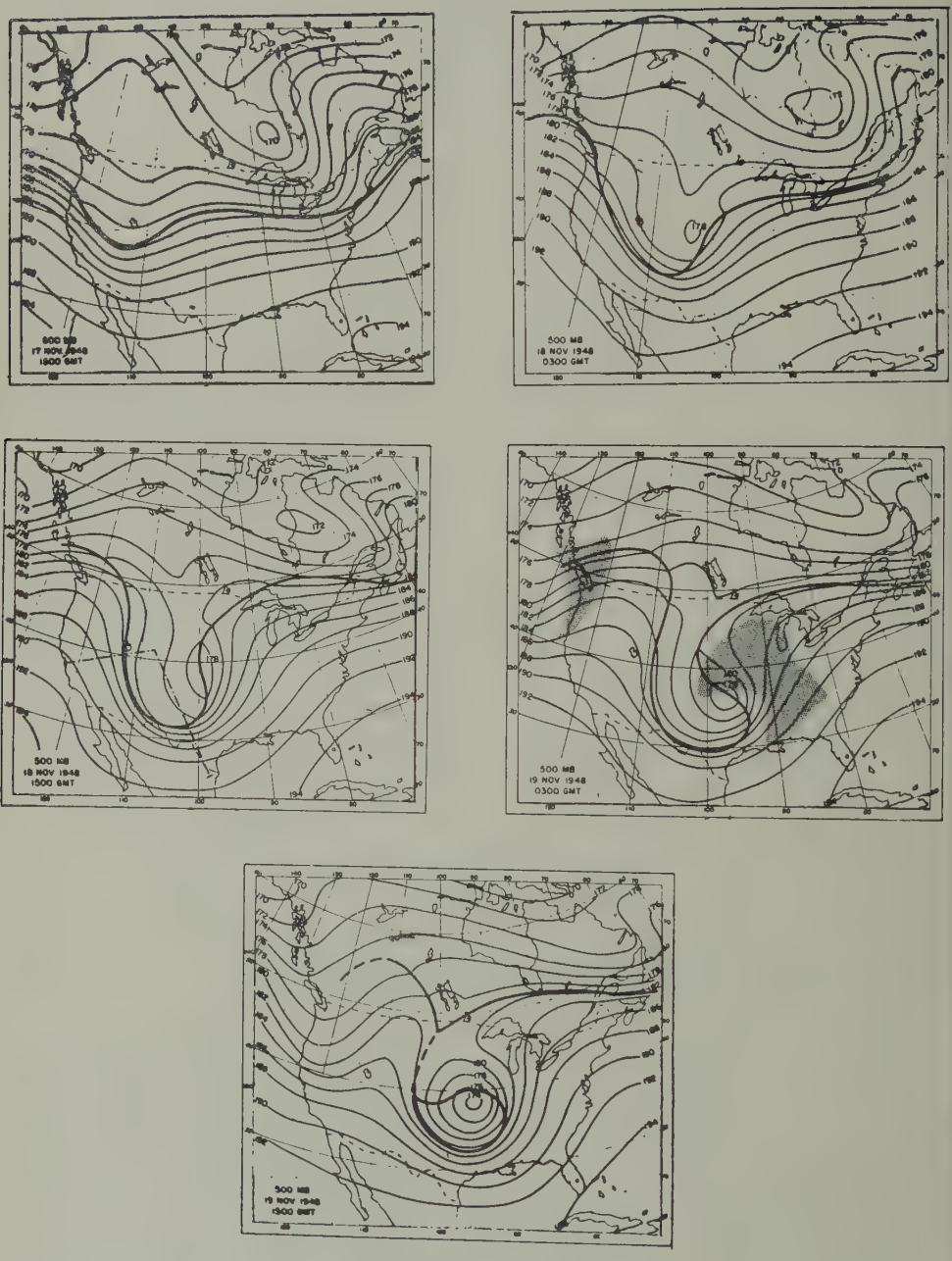

Figure 3. Example of the rapid development of an upper cyclone from a trough in the westerlies. Thin lines are contours of the 500 mb surface labelled in hundreds of feet. The thick line denotes southern boundary of the frontal zone. Order of charts : left to right and downwards.

even higher. The upper disturbance starting as a wave-shaped deformation of this polar front undergoes characteristic stages of development ; this development has been compared with a rapidly increasing ' meandering ' of a stream of air embedded in relatively stagnant air masses (Univ. of Chicago 1947). Often the deformation of the upper polar-front zone in the free atmosphere becomes so strong that large regions of cold air are cut off from the polar-air reservoir in the north. Fig. 3 gives an example of the rapid development associated with the development of an upper cyclone from a wave-shaped disturbance over North America.

During this development large polar-air masses flow southwards. Careful studies of typical cases, however, show that the area occupied by real polar air at the 500 mb level has a tendency to decrease after the initial outbreak, if the polar air has reached regions south of the mean latitude. This indicates that the polar air mass forms a cell of general subsidence.

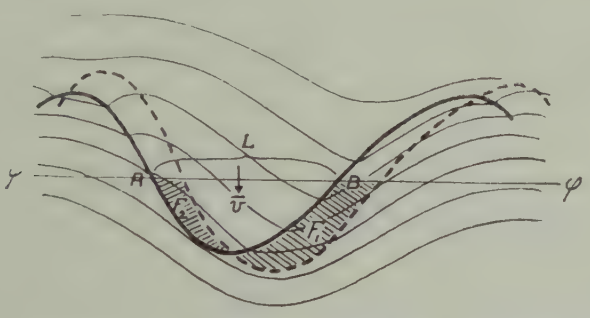

Figure 4. Schematic upper trough with front in two consecutive positions.

The descending motion in the southward-moving polar air can be computed if the boundary of cold air is a real surface of separation through which the air cannot move. The method of computation follows from the schematic Fig. 4. Here the thick line marks the front on an arbitrary isobaric surface at time t, and the thin lines denote the corresponding contour lines. The position of the front at a later time $t + \Delta t$ is marked by the dashed thick line. The latitude circle ϕ cuts the front in the points A and B with the distance L. The mean meridional wind component v between A and B is then approximately determined by the equation

$$\bar{v} \simeq -\frac{2\omega \sin \phi}{g}(H_1 - H_2) \quad . \quad . \quad . \quad . \quad (11)$$

and the change of the area of cold air C bounded by the latitude ϕ and the front is in the case of *horizontal flow*

$$\frac{dC}{dt} \simeq \frac{2\omega \sin \phi}{g}(H_1 - H_2) \quad . \quad . \quad . \quad . \quad (12)$$

where H_1 and H_2 denote the heights of the isobaric surface in A and B. This ' advective ' change can be compared with the observed change of area computed from consecutive charts ($F_1 - F_2$ in Fig. 4).

Suppose that ΔC_{II} is the ' advective ' change of the cold area C during the time

interval Δt and ΔC_I the observed change, then we have, because of conservation of mass,

$$\int_{p_h}^{p_0} \Delta C_{II}\, dp = \int_{p_h}^{p_0} \Delta C_I\, dp \quad . \quad . \quad . \quad (13)$$

where p_0 is the mean pressure at the earth's surface and p_h the pressure at the top of the cold air 'dome.' The total cold mass advection across latitude ϕ between A and B is

$$\Delta t \int_{z_0}^{z_h} \int_0^L \overline{\rho v}\, dx\, dz = -\frac{1}{g} \int_{p_h}^{p_0} \Delta C_{II}\, dp \quad . \quad . \quad (14)$$

and the cold mass transported through an arbitrary quasi-horizontal isobaric surface p during the same time Δt is

$$C\, \overline{\rho u}\, \Delta t = -\frac{1}{g} \int_{p_h}^{p} (\Delta C_{II} - \Delta C_I)\, dp \quad . \quad . \quad . \quad (15)$$

Computations of this kind have recently been made by Palmén and Newton (1951) in an especially favourable synoptic situation during 3 to 7 April 1950 over North America. In order to illustrate the situation a 500 mb chart for 4 April, 15 hr GMT is reproduced in Fig. 5 and a frontal contour chart for the same time in Fig. 6. The thermal structure in three sections (marked in Fig. 5 by three straight lines), and the geostrophic wind velocity normal to the planes of the cross sections can be studied in Fig. 7 which shows that the frontal surface is very distinct up to about 400 to 300 mb. Fig. 7 also shows that the front is a real boundary which is essential for the method of computation. From three consecutive sets of upper

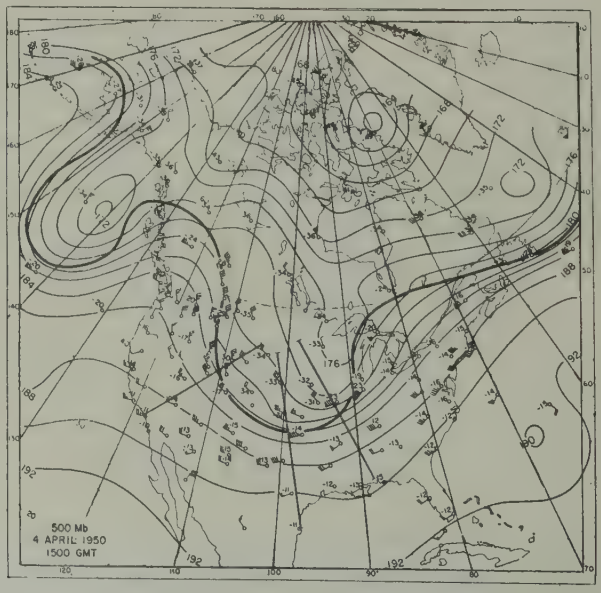

Figure 5. 500 mb chart for 4 April 1950, 15 GMT. Contours labelled in hundreds of feet. Heavy line represents 500 mb contour of polar front. Temperatures in °C, wind in knots (full barb 10 kt, heavy triangle 50 kt).

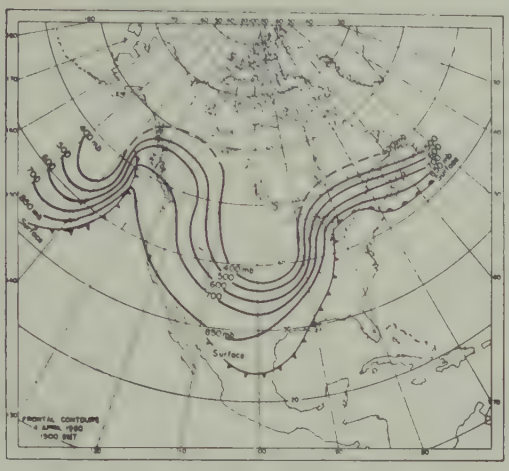

Figure 6. Frontal contour chart for 4 April, 1950, 15 GMT.

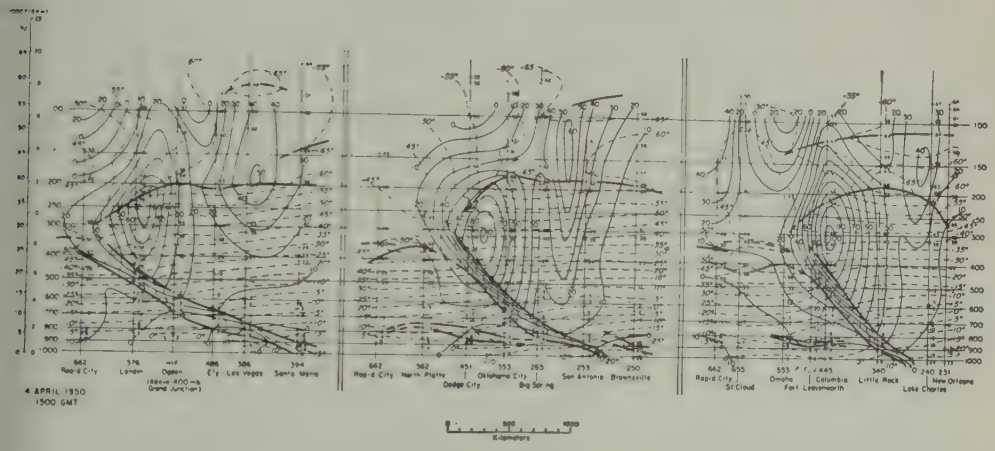

Figure 7. Cross sections along lines indicated in Fig. 5. Frontal boundaries and tropopause surfaces shown by heavy lines. Dashed thin lines are isotherms (°C) ; solid thin lines, isopleths (m/sec) of geostrophic wind normal to cross sections.

charts 1000, 850, 700, 600, 500 and 400 mb the cold air advection across latitude 45°N., the advective change of the cold areas and the actual change of the same areas were computed. The result of the computation is presented in Table II, the 300 mb level being regarded as the upper limit of the cold air. As can be seen from the table the maximum downward transport appears between 700 and 800 mb whereas the maximum mean velocity, due to the change of the cold area and density with height, can be found around 600 mb. This level is then also the approximate level of minimum horizontal divergence separating the upper convergence layers from the lower layers of divergence. In Fig. 8 some three-dimensional 36-hr air trajectories are presented which give an idea of the air flow in the cold outbreak.

TABLE II. MEAN DOWNWARD MASS TRANSPORT AND VERTICAL VELOCITY IN A COLD AIR MASS S. OF LAT. 45°N. DURING A PERIOD OF 24 HR, 4 TO 5 APRIL 1950

Pressure mb	Area of cold air (km^2)	Downward mass transport (ton day^{-1})	Mean vertical velocity (cm sec^{-1})
400	$1{\cdot}1 \times 10^6$	$0{\cdot}78 \times 10^{12}$	−1·4
500	1·8	2·20	−2·0
600	2·3	3·64	−2·2
700	3·5	4·59	−1·6
800	5·0	4·46	−1·0
900	6·7	2·67	−0·4

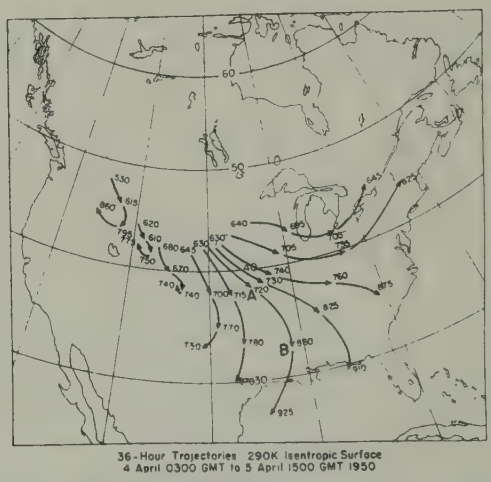

Figure 8. Trajectories for air parcels with a potential temperature of 290° during 36-hour period from 4 April, 03 GMT. to 5 April, 15 GMT. Pressure corresponding to different air parcels indicated in mb at 12-hour intervals.

The southward transport of cold air across latitude 45°N. in the cold outbreak studied here was computed from Eq. (14) to be $8{\cdot}3 \times 10^{12}$ tons for the 24 hr period. This value is about 1·1 per cent of the total mass north of parallel 45°N. If we assume that 5 similar disturbances or cold outbreaks are active at the same time their transport capacity for polar air would be about 5 per cent of the total air mass north of the latitude in question. It is obvious that similarly large warm air masses must replace this loss of polar air. That means a mass exchange of the order of magnitude 4×10^{13} tons day^{-1} between the 'polar' atmosphere and 'tropical' atmosphere around latitude 45°N.

The southward flow of genuine polar air takes place irregularly in close connection with polar-front disturbances, that is, as a discontinuous expulsion of large masses of air from the polar source region. The process is schematically described in Figs. 9 and 10 where four characteristic phases of the phenomenon are presented. Fig. 10 describes the process in meridional cross sections along the lines marked on the corresponding four schematic surface maps in Fig. 9. Both figures show the

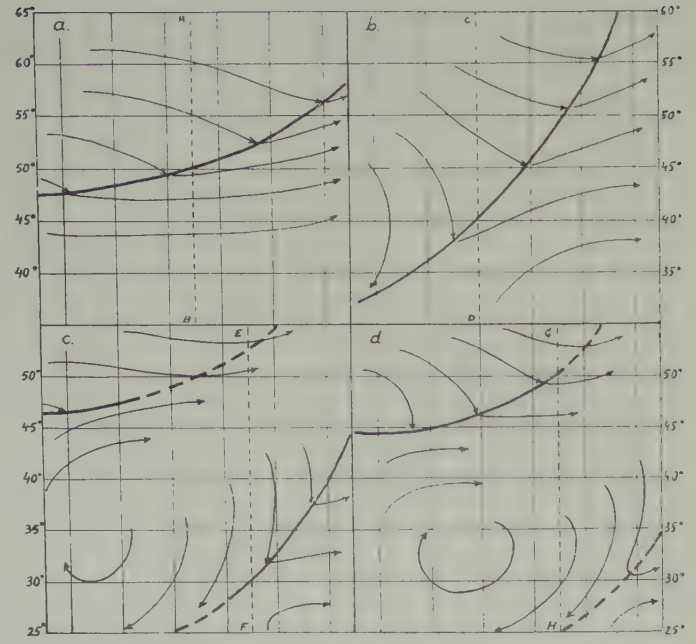

Figure 9. Schematic surface maps with fronts.

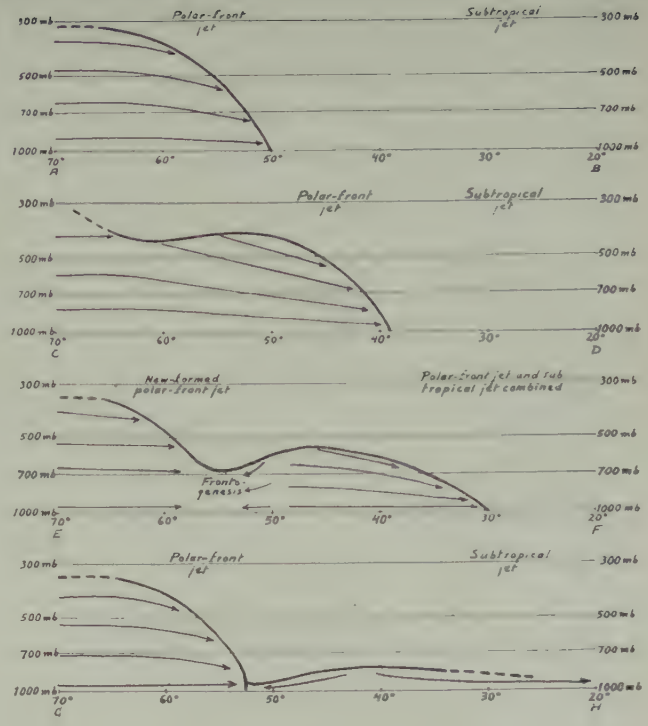

Figure 10. Schematic cross sections.

Figures 9-10. Four different phases of a cold-air outbreak.

successive positions of the polar front and the jetstreams. The sinking polar air tends to move southward along trajectories where the vertical component of vorticity decreases owing to horizontal divergence ; consequently the southern edge of the descending polar air reaches the trade wind zone as a relatively shallow and zonally limited anticyclonic stream embedded in tropical air masses. Trajectories of this type have been computed and described by Palmén and Newton (1951).

The discontinuous outflow of polar air according to Figs. 9-10 can be described in the following way. In the first phase there is one polar front, and the principal air motion is west-east. Connected with the polar front a pronounced jetstream is developed with its maximum velocity around the tropopause level. Farther to the south there is another jetstream corresponding to the subtropical jet described previously. In the second phase a rapid southward movement and increasing subsidence of the polar air sets in. In the third phase the polar air has subsided so much that a new upper front develops north of the principal front ; and with the continuation of the process the shallow cold air south of the new-formed front gradually loses its polar-air properties, as described in the schematic figures. In phase 3 the polar-front jet almost coincides with the permanent subtropical jet and forms an extremely strong current on the south side of a cold upper trough ; in this situation, however, a new jet forms over the region of the new-formed polar front.

It might be pointed out that the scheme presented here is in agreement with Cressman's (1950) and Phillips's (1950) results concerning the average southward displacement of individual jetstreams and the formation of new jets far to the north.

Analyses of the three-dimensional movements of the polar air thus give a picture of the flow pattern which is rather different from the common idea of a large-scale quasi-horizontal swaying around a mean latitude. The meridional amplitude of real trajectories seems to be much larger than the amplitude of upper stream lines, and the vertical component of the movement appears as an essential link in the whole development. Because of the great difficulties in following the three-dimensional movement of tropical air parcels we have neglected this other important branch of the extratropical circulation and mass exchange. However, there is no doubt about the fact that there must be a discontinuous inflow of tropical air masses into the polar region and that these, on the average, must ascend. Well-known phenomena such as the occlusion process, the distribution of precipitation areas, and the invasion of tropical air masses in the upper troposphere and lower stratosphere in very high latitudes in connection with the formation of strong disturbances show a discontinuous injection of upper tropical air in the polar region as a counterpart to the outflow of polar air in the lower part of the troposphere.

6. Scheme for the tropospheric mean meridional circulation and mass exchange

It is doubtful whether the mean meridional circulation and mass exchange can be represented by means of one meridional cross section only. Any such mean cross section is necessarily misleading in some respects. On the other hand some features at least of the general circulation can be elucidated by means of it and in spite of its deficiencies.

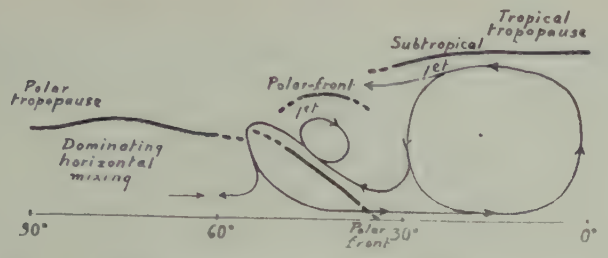

Figure 11. Scheme for mean meridional circulation during winter.

In Fig. 11 an attempt has been made to describe some of the essential features of the tropospheric meridional circulation viewed statistically. In this schematic figure which refers to the northern winter the circulation has been divided into three cells : the tropical cell, the extratropical or polar-front cell and the polar or subpolar region. The tropical cell is the principal source region of angular momentum and appears in our scheme essentially in the classical shape. At the northern end of this cell the quasi-permanent subtropical jet can be observed.

The meridional circulation associated with the polar front is much more complicated. The polar-front jet and the characteristic shape of the tropopause in the space between the real tropical tropopause to the south and the polar or arctic tropopause to the north indicate a circulation of the type presented in Fig. 11. The polar front is represented by a dashed line in the surface layers and in the upper troposphere in order to emphasize the necessity for an exchange of air across these latitudes. Experience also shows that there is generally a gap in the tropopause and that the polar front in the surface layers never appears as a continuous boundary around the whole hemisphere. The position of the mean polar front relatively far to the south corresponds to winter situations which on the whole are more distinct.

North of the polar front the transformation of the incoming branch of former tropical air takes place ; this is a region of subsidence associated with upper convergence and lower divergence. In the belt of upper convergence north of the jet the absolute vorticity often exceeds the polar vorticity. Around latitude 60°N. a region of convergence has been suggested ; this belt of polar convergence has often been wrongly regarded as the polar front. The whole region between the North Pole and latitude 60°N. is marked as a region where large-scale horizontal mixing processes are dominant ; that is the region of quasi-constant absolute vorticity (Rossby 1947).

It might be pointed out that no stratospheric circulation has been included in our scheme. Probably the circulation here is characterized by a weak poleward transport in the lower stratosphere and a similarly weak equatorward transport above that. Such a circulation is in agreement with the well-known fact that the relative rotation decreases upward with the exception of the polar stratosphere in winter. Finally it should be pointed out that the polar-front circulation in the form presented in Fig. 11 could never be found from mean data computed in the common fashion ; if however one referred the motions to the polar front at its mean latitude the characteristic circulation would more readily appear.

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The warming of arctic air masses over the eastern North Atlantic

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SUMMARY

Methods are described whereby 28 measurements were made of the rates at which cold air masses crossing the eastern North Atlantic were receiving heat and water vapour from the sea. It is shown that the rate of heat transfer is closely associated with the difference between the sea temperature and the surface temperature representative of the air mass, and some of the implications of this result are discussed.

1. INTRODUCTION

The local rate of change of mean temperature in any layer in the atmosphere, which may conveniently be expressed as the rate of change of the thickness of the layer between particular isobaric surfaces, can be resolved into components due to advection, to dynamic heating or cooling depending on mean ascent or subsidence in the air mass or to various non-adiabatic processes. Craddock (1951) has examined the part played by advection in thermal change in the lower troposphere, and has shown that when the advective component is large, it is generally offset by one of the others. This offsetting is often recognizable, qualitatively, on an upper-air (pressure-contour) chart, by the actual movement of the thickness lines between one chart and the next being in some regions less than that which would be deduced on the assumption that the thickness lines move with the airflow ; it is almost always apparent when cold air is advected across a warm sea surface. The warming of cold air masses over the sea is thus a matter of practical interest to the upper-air analyst, but is also of theoretical interest, as foreshadowed by Gold (1935), especially as the evidence adduced in the present paper goes to suggest that the warming is mainly a non-adiabatic process, and, in temperate latitudes, the most active non-adiabatic process to which an air mass can be subjected.

2. THE METHOD, AND THE DIFFICULTIES WHICH IT PRESENTS

The method is the simple one of finding the change in an air mass, following the airflow, by means of two upper-air soundings at different times and relating the changes observed to the conditions the air has experienced during the interval. In the present work, which is only one of several possible fields of application, all the air masses used were vigorous polar outbreaks and the initial and final soundings were made on opposite sides of an expanse of warm sea ; all the air masses gained heat in transit, and the rate of heating in each case was compared with the corresponding mean temperature difference between the sea and the air near the surface.

The main difficulty of the method is that while it is easy, by following the track of air which passes over one upper-air-sounding station at the time when a

sounding is being made, to find an air mass whose initial character is known, there is no way of ensuring that the air chosen passes anywhere near another upper-air-sounding station. In practice, a large number of promising occasions were examined, in which a broad cold airstream was moving over one of the northern radiosonde stations, *e.g.* Jan Mayen or Reykjavik, on a fairly straight track towards the dense upper-air network over the British Isles, but even so, most of the airstreams turned away before reaching the British Isles, so that the evidence here offered relates to a few fortunate occasions from which reliable information could be obtained, out of a much larger total. There are rather few weather observations on and around the sea area traversed and air masses can be followed across it only if the airspeed is neither too great nor too small and the trajectory is not excessively curved. If these conditions are satisfied, then any reasonable method of trajectory drawing should give much the same result as any other ; the methods described in the following section seemed to give the best return for the time expended.

A second difficulty is that the track followed by air which passes the initial sounding station, say, at the 700 mb level, will diverge from that followed by air passing the same station at the same time, but at a different level. This divergence, which depends on the wind shear between the levels, may amount to several hundred miles after the 24 hr or so taken by the sea crossing, so that a final sounding, which gives the condition of the air in the neighbourhood of one particular station will be strictly comparable with the initial sounding only at the level used for drawing the trajectory. Nevertheless, if there is a zone or layer in the air mass within which (i) the wind shear at each level is in the direction of the isotherms, (ii) the isotherms (and wind shear) have the same direction at all levels within the zone, and (iii) air at some one level within the zone can be tracked from the initial to the final sounding, then a fair comparison can be made between the two soundings, not only of the temperatures at the level used for tracking, but of those at any level within the zone. This is obvious on these conditions if we imagine a vertical cylinder traversing the zone and moving with the air which is actually tracked. The wind shear removes air from this cylinder, but replaces it by air at the same temperature, so that a comparison between the initial and final state of the air in the cylinder will show the changes which would have occurred if the air originally in the cylinder had been constrained to follow the same track at all levels. The first condition is a very familiar one, and would be expected to hold in the cases used, when the conditions necessary for reliable tracking imply that the accelerations of the air should be small, while the second condition will be fulfilled if the lapse rate is either constant or else a function of surface temperature only within the air mass. Now the present data and much else besides, suggest that an air mass subjected to long-continued convective heating does develop a characteristic lapse rate from near the surface up to the convective limit, as later defined, so that fair comparisons can be made in such air masses when the air is tracked at any level up to the convective limit. If, further, the air mass before convective heating started was homogeneous in the horizontal, it may be possible to use a trajectory at a level higher than the initial convective limit. Finally, in such air masses, the isopleths of water-vapour content tend to follow the isotherms, so that a comparison may be made of changes in water-vapour content as well as changes of temperature.

3. Methods of trajectory drawing

The routine upper-air charts prepared at six-hourly intervals at the Central Forecasting Office were used in the analysis. The trajectories were drawn on a transparent sheet which was transferred from one working chart to the next. In the first method the air at 700 mb only was followed, a trajectory being started at a northern radiosonde station at the time of a sounding, and the air being assumed to move with the local gradient speed for the three hours preceding and the three hours succeeding the time of each chart. Working thus, one chart was finished with before the next was looked at. Sometimes such a trajectory passed over a British upper-air station, but much more often it passed between the stations, or missed the British Isles altogether. When the trajectory passed between two stations, another trajectory was drawn starting at the same northern station at the hour of the next sounding and compared with the first. When the two trajectories lay closely alongside and included a British upper-air station between them, a trajectory relating to an intermediate hour was interpolated to pass directly over the British station. The method is thus one of interpolation at a fixed level with respect to time, and about 50 trajectories were drawn in this way.

In the second method, not only was the 700 mb trajectory drawn but also that for air at 1000 mb proceeding at gradient speed, and an estimate was made of the projection on the earth's surface of the air column which initially was vertically over the northern station. Since the projected segment joins isochronous points on the two trajectories and between them follows the run of the isotherms or thickness lines which appear on the analysed charts, the projection could be made with no more uncertainty than is inevitable in an analysis over a sea area. Sometimes the segment was extended by including the trajectory of air at 500 mb. If the conditions laid down in the preceding section are satisfied, any point on such a projection will also lie on a trajectory for some intermediate level, and if the segment passes across an upper-air station, the trajectory for the appropriate intermediate level can be interpolated. This method, which is one of interpolation at a fixed time between trajectories at different levels, is more fruitful than the other, and is susceptible to internal check, since simultaneous observations of different parts of a segment should and to a reasonable standard did confirm the uniformity of the air mass at different points on the segment. Of course, the part of the segment corresponding to air within the layer affected by surface friction could not be used – not, in practice, a serious restriction. Over 50 trajectories were drawn in this way.

Of the final total of over 100 trajectories, each starting over one upper-air station and finishing over another, some started and nearly all finished at times other than synoptic hours. Soundings for the initial and final times were constructed when necessary by linear interpolation at each level between the preceding and succeeding observations. (In finding moisture content, values of the humidity mixing ratio were interpolated). Only those cases were used in which the differences between the consecutive soundings were fairly small.

The drawing of trajectories by either method entails the exercise of personal judgment. All were drawn by the author personally. Some were rejected on internal evidence, and in order to standardize the conditions as far as possible, only trajectories starting at Reykjavik have been used in this paper. The 28 which are used

are, of course, not entirely unambiguous, but are considered to be reliable enough to show features which could be established by any experienced upper-air analyst who cared to repeat the investigation. Apart from the subjective element, since all the trajectories relate to air with fairly high surface velocity, say exceeding 15 kt, the results apply to conditions in moving air masses and not necessarily to semi-stagnant ones such as surround the sub-tropical anticyclones.

The cases used all related to the winter months between November and April ; the monthly frequencies are of no interest, as they depend on the occurrence of the particular circumstances in which observation is possible and may be expected to vary greatly from one year to another.

4. A survey of the data

The trajectories were drawn first and the soundings examined afterwards, when it appeared that the data fell into two main classes illustrated by Figs. 1 and 2. Transits resembling Fig. 1 were accepted as satisfactory. In some of these the air mass after crossing the sea had passed for a considerable distance over land and appeared to have been modified below the 900 mb level. The modification, which did not appear in transits ending at coastal stations, is typical of air over a land surface subject to cooling from below. The heating in these cases was assumed to take place entirely over the sea, so that the effective period of heating was less than the time between observations. The changes depicted in Fig. 2 were considered to indicate the presence of warm air overlying the cold outbreak, and hence a thermal wind changing direction with height. Any such in which the trajectory lay in the warm air were discarded, as violating the second condition laid down in Section 2. Five transits which are open to some doubt on this ground (as the trajectory seemed to lie in the frontal transition zone) have been included, as they were all that could be found for one particular outbreak.

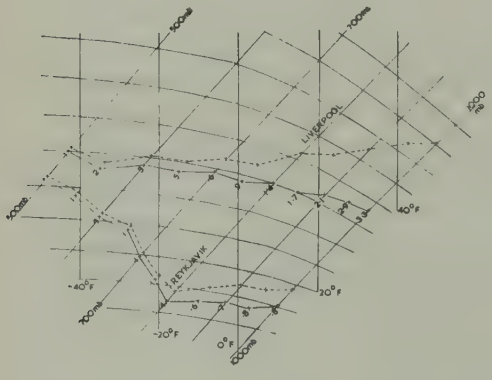


Figure 1. Reykjavik 2100 on 18 Jan. 1945 to Liverpool 2100 on 19 Jan. 1945. Actual temperatures in full curves. Equivalent temperatures in pecked curves. Figures give humidity mixing ratio in gm kg^{-1}.

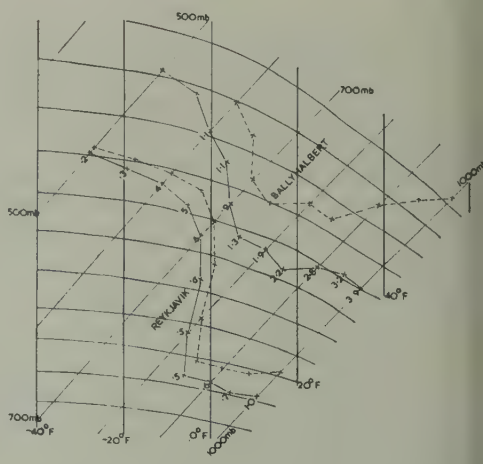


Figure 2. Reykjavik 1200 on 3 Feb. 1944 to Ballyhalbert (N. Ireland) 0330 on 4 Feb. 1944. Actual temperatures in full curves. Equivalent temperatures in pecked curves. Figures denote humidity mixing ratio in gm kg^{-1}.

Each of the air masses studied had received heat in the lower layers, as shown by comparing the initial and final temperatures. Further, each was, while over the sea, in a state of convective agitation, as indicated by reports of showers, gusts and hail at the coastal stations. It appears therefore that the depth of air directly responsive to changes in the surface conditions will be indicated by the depth of convective cloud, a depth which was sometimes reported directly by an aircraft sounding, but more often had to be deduced by means of some indicator which distinguishes an air mass subject to convective mixing from one which is not. The equivalent potential temperature θ_e was used for this purpose and proved satisfactory. In all cases θ_e was fairly uniform in the lower layers, and usually showed large increases with height above, so that the limit of the air mass subject to convective mixing could be determined to within 50 mb without much difficulty. Occasionally, soundings at neighbouring stations were consulted. The heights of the tops of convective cloud, when reported by aircraft, agreed with the top of the layer of nearly constant θ_e, and this level will be referred to as the convective limit.

In the transit shown in Fig. 1 the convective limit was at 800 mb when the air was at Reykjavik, but had risen to 550 mb by the time it reached Liverpool. A change of this kind occurred in nearly all the transits, and in some the final convective limit coincided with the tropopause. It appears that convection was at first shallow, but became deeper as the warming of the suface layers increased the lapse rate aloft.

The changes in temperature observed (*e.g.* in Fig. 1 an increase in 24 hr of 23°F at the 1000 mb level, 26°F at the 800 mb level and 7°F at the 550 mb level) are far beyond the range of observational error. A calculation was made for each case of the rate of gain of heat by a column of air of 1 cm^2 section extending from the sea surface up to the convective limit found from the final sounding ; it was assumed that the gain of heat per unit mass for air between, say, 875 mb and 925 mb is $c_p\,\delta T$ where δT is the actual increase in temperature at the 900 mb level between the initial and final soundings. Thus the computed figures for rates of heat gain include both the heat transformed into internal energy (measured by $c_v\,\delta T$) and the work done by the air at each level in lifting the air above. Typical and extreme values of this rate are given in cal cm^{-2} hr^{-1} and in mw cm^{-2} in Table I Col. (1).

Similar calculations based on the initial and final values of the water-vapour content of the air mass at each level give the values given in Table II Col. (2) for the rate of gain of water vapour by the air column. These latter figures are less than the actual values, because they take account only of water vapour passing into the air mass and retained by it and not of that which is evaporated into the air and later condensed and lost in the form of precipitation, and the occurence of showers at all the coastal stations shows that this loss was occurring. Water which is evaporated from the sea surface must derive its latent heat of evaporation from the sea, while if condensed later it imparts that latent heat to the air, so that water which performs this cycle plays a part in warming the air mass. We may in fact fix an upper limit to the rate of evaporation by observing that the latent heat of the water evaporated and later condensed and lost must not exceed the sensible heat gained by the air during the same period. Values for this upper limit are given in Table I Col. (4). Table I Col. (3) gives values for the total rate of gain of heat energy, including the

sensible heat gain under Col. (1) and the latent heat gain under Col. (2), and is, of course, the equivalent in heat units of the water-vapour gain under Col. (4). The heat made available to the air by the precipitation of 1 g of water depends somewhat on the conditions, expecially on whether the precipitation takes the form of water or of ice. Probably in fact some of each occurred, but in calculating Table I Cols. (3) and (4) it has been assumed that all precipitation took the form of rain so that the latent heat of condensation, but not that of freezing, became available to the air.

TABLE I. TYPICAL AND EXTREME RATES OF GAIN OF HEAT (COLS. (1) AND (3)) AND WATER VAPOUR (COLS. (2) AND (4)) BY ARCTIC AIR MASSES PASSING OVER WARM SEA

	Sensible heat (1)		Min. water vapour (2)	Total heat (3)		Max. water vapour (4)
	cal cm^{-2} hr^{-1}	mw cm^{-2}	mg cm^{-2} hr^{-1}	cal cm^{-2} hr^{-1}	mw cm^{-2}	mg cm^{-2} hr^{-1}
Typical	36	42	18	47	55	72
Extreme	65	76	29	79	92	133

Table I shows several points of interest. The value for the total rate of gain of heat by a typical Arctic air mass passing over warm sea, 47 cal cm^{-2} hr^{-1}, is very large compared with everyday experience. To have a standard of comparison, we may take the solar constant, 115 cal cm^{-2} hr^{-1}. This figure gives the upper limit to the rate at which an air mass can gain heat by direct insolation, a limit which can be approached in nature only under very special conditions. Incident sunlight mostly heats the ground surface in the first place, and in temperate latitudes the oblique angle of incidence reduces the heat energy available per unit area of ground. Further, much of this sunlight goes to warm the soil and is not re-transmitted to the air. Measurements of the proportion of incident solar energy which is returned to the air have been given, *e.g.* by Sumner (1947), and Groen (1947) quoting Franssila, and suggest that the proportion depends mainly on the conductivity of the soil, being less than half for most surfaces, but approaching unity for peatland. Sutton (1948) obtains a value of 11 cal cm^{-2} hr^{-1} for the flux of sensible heat into the air over a grass surface in the S. of England for a clear June day in the hours around noon. Pasquill (1949) has provided measurements of the evaporation from a clover pasture near Cambridge near noon on a sunny day in May, from which the maximum flux of latent heat into the air may be calculated to be 32 cal cm^{-2} hr^{-1}. The total of sensible plus latent heat, 43 cal cm^{-2} hr^{-1}, compares with our value for the rate of heat transfer to an arctic air mass over warm sea, but unlike the latter is confined in any 24 hours to a period of about three hours around noon.

It appears therefore that a cold air mass can be modified far more rapidly in temperate latitudes by passage over warm sea than by summer insolation.

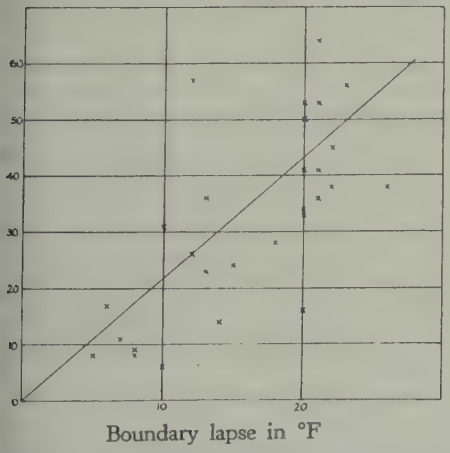
Reverting to Table I, the moisture which the air mass gains over the sea can hardly be derived from anywhere but the sea surface, and it is noteworthy that the maximum rate of evaporation, sufficient to supply as latent heat all the heat energy which the air mass gains, is only four times the proved minimum. Hence, any process envisaged to explain the latter could be made without much alteration to explain the former.

Finally, the difference between the two rates of evaporation gives an upper limit for the mean rate at which moisture in such an air mass is being condensed and deposited in the form of rain. The rate of rainfall works out at the moderate value of 0·5 mm hr^{-1} for the typical case and 1·0 mm hr^{-1} for the extreme case. This average value will be made up by local showers of much greater intensity which may or may not be distributed at random, with temporarily clear spaces between.

5. The relation between the rate of warming of an arctic air mass and the temperature change across the boundary layer at the sea surface

An estimate was made of the mean surface-air temperature during each transit (by taking the mean of the actual surface-air temperatures of the initial and final soundings, ignoring any small inversions) and this was subtracted from the mean sea temperature along the track, estimated from the mean isotherms for the month in question with a small adjustment based on synoptic experience for the distortion of the isotherms brought about by a steady airstream. This difference, which will be called the boundary lapse, is a rough guide to the intensity of the temperature change across the sea-air boundary layer.

The boundary lapse for each occasion is shown, plotted against (a) the rate of gain of sensible heat and (b) the rate of gain of water vapour for that air mass in Figs. 3 and 4. In each of these graphs there is a strong association between the variables. These relations form the most interesting results of the investigation, and at the same time, provide the most cogent evidence of the soundness of the technique. The errors which arise in tracking and timing the air masses, or in estimating the convective limit might lead to the concealment of an association between quantities measured independently, but hardly to the appearance of a relation where no real relation existed. In view of the association shown in Fig. 3,



igure 3. Relation between the rate of warming of a cold air mass and the temperature change across the sea surface.

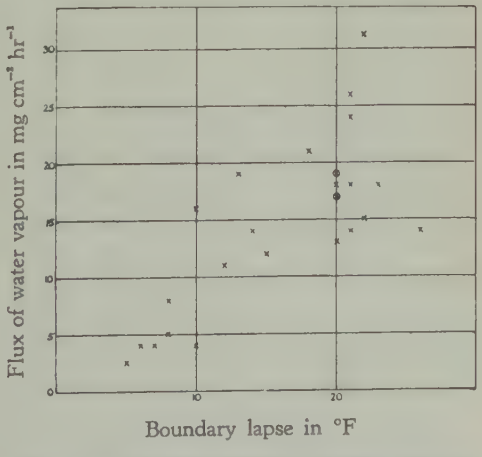


Figure 4. Relation between the rate at which a cold air mass is gaining water vapour and the temperature change across the sea surface.

the boundary lapse can be used to estimate the rate of heating by means of a regression equation. Since, however, this relation is of value to the upper-air analyst, it is convenient to state it in a form suitable for practical use, namely : *If the boundary lapse is n°F and the convective limit not above 700 mb, then the thickness of the layer from 1000 mb to 700 mb will increase by n ft hr*$^{-1}$. This rule corresponds to the relation between boundary lapse and flux of sensible heat shown in Fig. 3 by the sloping line, which is a good although not quite the best fit for the observations. When the heating extends above 700 mb, the increase in thickness given by the above rule will have to be divided between the layers affected. If the analyst is concerned only with the thickness of the 1000-500 mb layer, the thickness given by the above rule may be increased by an arbitrary amount of say 10 per cent. This is to allow for the facts that when there is deep convective heating the increase in thickness per calorie is rather greater for the heat reaching the upper levels than for that heating the lower levels, and that when convection is shallow there will very often be advective heating above of the type illustrated by Fig. 2.

6. A discussion of the results

These results have a bearing on much published work, mostly American, which, however, differs considerably from the present work in object or method. It would be a considerable undertaking to pass this work under critical review, and this will not be attempted here. The present section deals only with what may reasonably be inferred from the present data.

The relations shown in Figs. 3 and 4 suggest strongly that the rates of gain of heat and water vapour by the air mass are dependent on conditions in the boundary layer, a dependence which is intelligible if and only if heat and water vapour are in fact passing across the boundary into the air. Nevertheless before accepting this view we should consider all the processes by which an air mass can gain heat or water vapour, namely, the following :

(*a*) *Advective heating*

The fact that the soundings compared relate to the same air at one particular level rules out the possibility of advective heating at that level, and within any zone in which the conditions laid down in Section 2 are satisfied. At levels where these conditions are not satisfied, particularly levels where the direction of the thermal gradient differs from that of the level for which the trajectory was drawn, advective heating can and does occur, as illustrated in Fig. 2 by the warming in the upper levels which took place between the two soundings. It is not considered that this form of heating operated in the layers up to the convective limit in any of the cases used here.

(*b*) *Dynamic heating*

It is easy to show that continued subsidence of moderate intensity is capable of producing temperature changes of the magnitude observed here, and the view is commonly held that cold air masses moving south are generally subsiding and

hence subject to dynamic heating. We cannot here review all the evidence in favour of this view, which is undoubtedly correct in many cases, but it is worth mentioning that one class of evidence in support, namely, that based on computations of the vertical velocity by the field method, assuming the conservation of the potential temperature, is in fact misleading, since the method implicitly attributes any non-advective temperature change of the air mass to subsidence or ascent. It may be agreed that nearly all cold air masses moving south do in fact warm in transit, but the question whether this heating is due to subsidence should surely be decided on the merits of each case and not by reference to any supposedly universal rule.

The evidence in the present cases is against the hypothesis that the heating observed is mainly dynamic. Subsiding air masses are generally characterized by declining convection and clearing skies and are very much drier in the upper troposphere than in the surface layers, whereas the air masses here considered were all maintaining violent convection as long as they were over the sea and show about the same relative humidity at all levels up to the convective limit.

(*c*) *Non-adiabatic heating by lateral diffusion*

This process is significant on the hemispherical scale on which depressions and anticyclones can be regarded as ' eddies,' and possibly on a smaller scale in frontal regions of strong wind shear and intense thermal contrast, but it can hardly be important here, in changing the character of an air mass showing very moderate gradients of wind shear and temperature.

(*d*) *Radiative effects*

These cannot be discussed with any exactness, for they depend on the water-vapour content and cloud structure in the air masses during the sea crossing, highly variable quantities on whose values we have little direct evidence. Nevertheless, we may judge their order of magnitude, taking for example the typical conditions illustrated in Fig. 1, and assuming that the effective radiating temperature of the sea surface increased from 33°F near Reykjavik to 49°F near Liverpool. The radiative fluxes are estimated from the actual data with the Kew radiation chart, described by Robinson (1947). This chart has been shown to give reliable estimates of the fluxes at levels near the ground surface, but a good deal of uncertainty attaches to estimates made with this or any other chart of fluxes at levels in the upper troposphere. Two alternative cases (Table II) are considered, (a) of clear skies and (b) of a cloud sheet at — 20°F with clear skies above and below.

The figures are subject to uncertainty, because of the very low temperatures in the upper troposphere, which make the measurements of atmospheric humidity unreliable and which lie outside the proper range of the radiation chart. Nevertheless, they suggest that within the range of temperature considered the air up to 500 mb is losing heat by radiation, whether it is cloudy or clear, at an average rate less than 8 mw cm^{-2}, or 7 cal cm^{-2} hr^{-1}. The correction for radiative effects thus amounts to a small addition to the values in Table I for the rate at which the air is receiving heat by other processes.

TABLE II. RADIATION BALANCE FOR THE LAYER, SURFACE TO 500 MB

(a) Clear skies	At Reykjavik 18 Jan. 1945 (mw cm^{-2})	At Liverpool 19 Jan. 1945 (mw cm^{-2})
Upward flux at sea surface	30	33
Downward flux at sea surface	15	22
Net upward flux at sea surface	15	11
Upward flux at 500 mb	25	26
Downward flux at 500 mb	6	7
Net upward flux at 500 mb	19	19
Net rate of loss of energy by layer	4	8
(b) Cloud sheet at −20°F		
Upward flux at sea surface	30	33
Downward flux at sea surface	22	28
Net upward flux at sea surface	8	5
Upward flux from cloud top	20	20
Downward flux on cloud top	8	9
Net upward flux from cloud top	12	11
Net rate of loss of energy by layer surface up to cloud top	4	6

(*e*) *Water vapour derived from falling rain*

Where a warm front overlies the cold air, rain falling through the cold air may evaporate and raise the water-vapour content of the latter. The total heat content will remain unaltered, however, since the latent heat of evaporation must be derived from the air. This process can hardly be responsible for the simultaneous increase in water-vapour content and in total heat content which is observed, and furthermore, in several of the examples of very rapid heating the cold air extended right up to the tropopause without the intervention of any air of different origin.

(*f*) *Direct evaporation from the sea surface*

When evaporation takes place from the sea surface (but not when it occurs from flying spray) the latent heat of evaporation will be derived from the sea. The hypothesis that when a cold air mass moves over warm sea it gains heat and moisture mainly as a result of intense evaporation from the sea surface seems to accord with all the facts here presented.

ACKNOWLEDGMENTS

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The modification of continental polar air over Hudson Bay

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Summary

Changes in the physical properties of continental polar air are studied for air currents with trajectories over the Hudson Bay region. Computations indicate that westerly air streams crossing Hudson Bay in early winter are modified to a depth of 5 to 10 thousand ft. By midwinter, however, the modification is found to be negligible. This lack of modification in midwinter is attributed to the formation of an ice surface on Hudson Bay. Changes in the climate of the surrounding area caused by the formation of ice on Hudson Bay are illustrated by changes in temperature, cloud and snowfall. The modifying influence of the Bay in cooling the air in summer and heating the air in early winter is also discussed.

1. Introduction

The two main factors in the modification of continental polar air over Hudson Bay are : (a) the temperature of the surface water, since this acts as a temperature control, and (b) the amount of ice that forms in winter, since this reduces the water area available for the modification of air. Once these two factors are computed the modification can be correlated with the known properties of the modifying surface. It is the purpose of this study : (1) to compute the changes in the physical properties that occur in continental polar air over Hudson Bay ; (2) to determine the effect of the formation of sea ice in changing the modifying influence of the Bay, and conversely to prove from meteorological evidence that Hudson Bay becomes covered with ice in winter ; (3) to evaluate the climatic influence and meteorological significance of Hudson Bay.

The modification of continental polar air over a water surface has been studied by Burke (1945), Klein (1946) and others over the North Atlantic. A similar method of computing air trajectories and comparing the radiosonde ascents before and after modification by a water surface was applied to the case of air streams crossing Hudson Bay. It became apparent, however, that the Bay does not act as a normal oceanic heat source in midwinter. This pointed to a change in state of the modifying surface from water to ice in the midwinter season.

2. Hudson Bay surface-water temperatures and ice conditions

The temperature (freezing point) of the surface water of Hudson Bay in winter can be taken as 30°F. Near-freezing temperatures continue through spring into summer. A rapid increase in temperature occurs by late summer with reports from ships indicating 50°F in southern sections of the Bay and 40°F in northern parts. The great range of temperature between summer and winter is mainly due to the

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flood of fresh water from the many rivers that drain into Hudson Bay. During the fall and early winter the residual heat of the water is lost in modifying the cold continental polar air.

Until 1948 little was known about the area of ice in the central waters of Hudson Bay in winter. Ice was known to form around the edges but reports by local residents and explorers all expressed the opinion that the central parts of Hudson Bay remained as open water throughout the winter. This was also the opinion of many circles in the United States ; the authoritative *Ice Atlas of the Northern Hemisphere* (1946) shows only a narrow coastal fringe of ice. A review of these earlier opinions has been given by Hare and Montgomery (1949).

Preliminary investigations in 1947 on the modification of continental polar air while crossing Hudson Bay indicated little change in properties in midwinter. This result was attributed by the author to the formation of an ice cover. Photographic evidence supporting this conclusion was obtained on ice reconnaissance flights in 1948, 1949 and 1950 by the Royal Canadian Air Force. The author took part in the " Ice Cake " and " Cariberg " flights in April and May 1948. On these flights Hudson Bay was covered with pack ice. The only open water was in narrow coastal channels along the lee shore, and in the holes and cracks of the pack ice. For accounts of the ice conditions encountered on the various reconnaissance flights reference can be made to reports by Hare and Montgomery (1949), Lamont (1949), Burbidge (1949) and others. Observations show that Hudson Bay was covered with pack ice in the winters of 1947/8, 1948/9 and 1949/50.

3. Results from climatic data

(*a*) *Temperature Comparison*

The mean monthly temperatures at Port Harrison (58° 28′N., 78° 08′W.), and at Churchill (58° 45′N., 94° 07′W.), at similar latitudes but on opposite sides of Hudson Bay, are shown in Table I. Since westerly winds occur more frequently than easterlies over this region the modifying effect of Hudson Bay is greater along the east coast than along the west coast. Consequently, if the Bay remains open during the winter the mean monthly temperatures at Port Harrison should be higher than at Churchill throughout the winter months. From Table I it can be seen that marked modification of the air does occur in early winter (Port Harrison is eleven degrees warmer than Churchill in November). By midwinter, however, there is no longer any modifying effect. This seems to indicate that the Bay freezes over. It may also be noted that the modification becomes negative in summer. The mean monthly temperatures at Port Harrison are lower than those at Churchill owing to the cooling influence of the Bay.

TABLE I. Mean monthly temperatures (°F) at Churchill, Man. and Port Harrison, Que.

	S	O	N	D	J	F	M	A	M	J	J	A	Years of Observation
Churchill	42	27	6	−11	−19	−17	−6	14	30	43	54	52	30
Port Harrison	41	31	17	− 2	−18	−18	−7	11	29	39	47	47	18

(*b*) *Temperature under onshore and offshore winds*

The average temperatures for air arriving at Port Harrison from the west off Hudson Bay and from the east off the land are given in Table II for the period 1942/4. The typical marine influence of cool onshore winds in summer and warm onshore winds in winter is indicated with one exception. In the midwinter period when warming by the relatively warm water should be most effective, there is none. The absence of modification in midwinter suggests that the Bay is covered with ice. Pronounced warming occurs in November whereas cooling occurs throughout the summer.

TABLE II. AVERAGE TEMPERATURES (°F) AT PORT HARRISON UNDER ONSHORE AND OFFSHORE WINDS (1942/4)

	S	O	N	D	J	F	M	A	M	J	J	A
West winds (onshore)	40	32	26	0	−14	−16	−10	14	26	34	42	48
East winds (offshore)	45	30	13	−3	−10	−14	− 7	16	27	39	53	53

(*c*) *Snowfall*

The modification of cold continental polar air by Hudson Bay in early winter results in considerable snow. The air is extremely unstable after passing over the Bay owing to the addition of moisture and heat in the lower levels. Almost continuous snow results in the coastal sections with onshore winds. This snowfall is analogous to the Great Lakes Snow Belt and the Great Snow Belt of Japan. The prevailing north-westerly wind over the Hudson Bay region results in greater amounts of snow along the east coast than along the west coast. In addition, the rapid rise into the Ungava Plateau of northern Quebec intensifies the snowfall along the east coast. The greatest amount of snow at Port Harrison falls during the month of November (33 in.). From this maximum in early winter, the snowfall decreases rapidly to negligible amounts in February (only 2·8 in.). This lack of heavy snowfall in midwinter is meteorological evidence indicating that the cold air is insulated from the water of Hudson Bay by the formation of an ice surface.

(*d*) *Cloud*

A double cloud-maximum, one in May and the other in November, occurs in the Hudson Bay region. In the spring, stratus cloud forms in the warm, fairly moist air streams passing over the cold surface of Hudson Bay. This cloud is not a particularly prominent feature of modification of air by the Bay because the whole of northern Canada presents a melting snow and ice surface in early spring, and this cloud is typical of the entire area. After the snow has melted, however, the ice and cold water of Hudson Bay tend to make this advection stratus persist into the summer season in the region of Hudson Bay. One important effect of this cloud is in extending the cooling effect of Hudson Bay over a large area in summer. Orographic cooling is an additional factor in forming and maintaining this cloud over the surrounding region.

In winter, excessive evaporation from the relatively warm water into the cold air results in steam fog and stratocumulus cloud over northern open-water areas. This is true of Hudson Bay in early winter, but there is a general clearing of this cloud in the Hudson Bay region by midwinter. Steam fog still occurs over holes in the ice but the water area is so small that no extensive cloud forms. The disappearance of this cloud by midwinter coincides with the disappearance of open water in Hudson Bay.

4. Modification of surface air temperature

Individual cases of steady westerly air streams were studied to determine the specific changes that occurred during the trajectory of the air over Hudson Bay. The parallelogram method modified by weighted dynamic effects was used to determine the trajectory of the air. Continental polar air leaving the western side of Hudson Bay with a temperature T_0 is modified in crossing the Bay, and arrives on the eastern shore with a temperature T_m. The amount of modification was obtained by comparison of these two temperatures. In most cases the time for the air to cross the Bay was approximately 24 hr.

(*a*) *November*

The surface synoptic charts for the four years, 1945 to 1948, were reviewed and 89 cases were found with appropriate air streams crossing Hudson Bay and arriving over Port Harrison.

The modified temperature T_m at Port Harrison was plotted against the original air temperature T_0 to illustrate the amount of modification by Hudson Bay (Fig. 1).

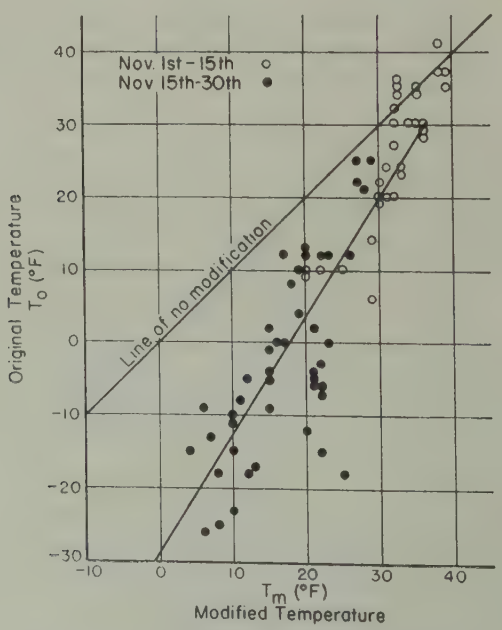

Figure 1. Surface-temperature modification in air streams crossing Hudson Bay in November.

The equation of the best-fitting line for the points on the scatter diagram in F is

$$T_m = \cdot 6T_0 + 17\cdot 6 \quad . \quad . \quad . \quad . \quad (1)$$

nd the equation for the amount of modification is

$$(T_m - T_0) = 17\cdot 6 - \cdot 4T_0 \quad . \quad . \quad . \quad (2)$$

Of the 89 cases in Fig. 1, 85 per cent. are within 6°F of the best fitting line. The high correlation indicates that T_m is a function of T_0. However, the modified emperature T_m also depends on other factors such as the original lapse rate of the ir and length of trajectory over water.

Marked warming of the continental polar air occurs in the month of November. The modification increases during the month as indicated by the points on the catter diagram. The amount of modification is small during the first half of November when continental polar air masses do not have temperatures much below 2°F. As the air masses become colder the modification increases until by the end f November the heating amounts to over 20°F.

(*b*) *December*

The surface synoptic charts for the four years 1945 to 1948 were reviewed nd 64 cases were found with appropriate air streams crossing Hudson Bay and rriving over Port Harrison. For each case, the trajectories were drawn back to he west side of the Bay and the original temperature was determined. The tem- erature after modification for each case was plotted against the original temperature n a scatter diagram. The points fell into groups by years with wide dispersion naking it impracticable to compute a best-fitting line or curve. The average modi- ication, however, was calculated and the results are presented in Table III.

TABLE III. The average surface-temperature modification in December

	T_0 (°F)	T_m (°F)	Modification (°F)
December 1 to 15	— 8	+ 12	20
December 15 to 31	— 20	— 10	10
Average for Month	— 14	+ 1	15

It is plain that modification of the cold continental polar air by Hudson Bay ontinued during the month of December. In two cases, however, no modification ccurred, and in three cases the Port Harrison temperature was lower than the riginal temperature.

The results for individual cases varied widely from the average values given n Tables III and IV. The erratic nature of the modification in the month of December can be largely attributed to two factors : (a) the variation in the amount f ice and (b) the variation in T_0 from one year to the next.

The noticeable decrease in the amount of modification during December indicates that the Bay is frozen. Cold air passing over a warm ocean is rapidly modified ; with other factors constant, the colder the air the greater the modification. In the case of Hudson Bay, this rule held during the month of November ; as the continental polar air masses became colder, the modification increased. During December, however, as the air became colder the amount of modification decreased. This indicated that Hudson Bay no longer had the same modifying power and pointed to a decrease in the area of open water.

(c) *January, February, March, April and May*

Typical cases of modification in air streams crossing Hudson Bay in the midwinter and early spring are shown in Table IV. Some temperatures of the air after crossing Hudson Bay are higher than the original, some are lower. In general, air currents from the north-west show slight warming, whereas westerly or south-westerly trajectories show little change in temperature. If the areas of open water that are indicated in the Ice Atlas were present in winter, the warming would amount to over 30°F. The computed values in Table IV indicate negligible modification.

The lack of modification in midwinter can be directly attributed to the formation of ice. The positive modification that does occur can be accounted for by warming over the small areas of open water exposed through cracks, holes and channels in pack ice, and by heat transfer through the ice.

TABLE IV. Typical cases of modification in air streams crossing Hudson Bay in winter
(Each case represents the grouping of points on a scatter diagram)

Month	Trajectory Path	T_0 (°F)	T_m (°F)	Modification (°F)
Dec.	—	4	11	7
		− 8	12	20
		−10	20	30
		−13	3	16
		−17	− 5	12
		−33	−22	10
		−20	−10	10
Jan.	NW.	−30	−20	10
	W.	−20	−17	3
	SW.	− 8	− 8	0
Feb.	NW.	−35	−25	10
	W.	−22	−20	2
	SW.	−14	−14	0
Mar.	NW.	−22	−14	8
	W.	− 6	− 4	2
	SW.	− 3	− 3	0
Apr.	NW.	− 7	− 5	+ 2
	W.	5	3	− 2
	SW.	18	18	0
May	NW.	12	17	5
	W.	28	28	0
	SW.	36	32	− 4

(d) *Summer*

During the summer the continental polar air masses have surface air temperatures generally higher than the water temperature of Hudson Bay. The warm air streams crossing the Bay are cooled or negative modification occurs. Results of typical cases are given in Table V.

The amount of modification was found to depend on the direction or path of the air stream and on the time of origin of the trajectory. North-west winds from the arctic were only slightly affected in passing over Hudson Bay. These cool air streams can reach northern Ontario, Quebec and Manitoba in an unmodified state. The diurnal temperature variation in returning south-west circulations of warm continental polar air caused large variations in the modification. Air trajectories originating in mid-afternoon showed the greatest modification. Radiative cooling at night caused the temperatures in the air before crossing Hudson Bay to approach the Bay temperature. Hence cases originating at night showed less cooling over the sea. By September the flood of warm fresh water from the many rivers that drain into Hudson Bay had increased the Bay temperature to values comparable to the average temperature of the air with the result that there was little net modification during the autumn months.

TABLE V. Typical changes in temperature (°F) of air streams crossing Hudson Bay in summer

	Returning SW. circulation of cP air (afternoon)			Returning SW. circulation of cP air (night)			NW. circulation of cP air		
	T_s	T_m	Mod.	T_s	T_m	Mod.	T_s	T_m	Mod.
May	36	32	− 4	28	28	0	12	17	5
June	50	38	−12	40	34	− 6	25	26	+ 1
July	60	44	−16	48	42	− 6	40	38	− 2
August	60	48	−12	50	45	− 5	41	40	− 1
Sept.	52	48	− 4	44	44	0	30	36	+ 6
Oct.	48	42	− 6	38	38	0	22	33	+11

5. Changes in the vertical structure in continental polar air streams crossing Hudson Bay

Changes in lapse rate and the depth of air modified in crossing Hudson Bay were studied by analysis of upper-air maps and by comparison of soundings taken in the air before and after modification. The simplest case was a steady westerly air stream from Churchill to Port Harrison which had little or no shear with height. In other cases, the air column arriving over Port Harrison was considered to be made up of parcels of air at different levels which, because of the shear of the wind with height, had originated over different places on the western side of the Bay. Trajectories were therefore traced back from Port Harrison to the western side of the Bay for the surface, 850, 700 and 500 mb levels and a composite sounding was constructed for the original cP air.

(a) *Early winter*

The normal lapse rate in continental air masses in winter is very stable, usually a pronounced inversion throughout the lower levels. This stable lapse rate is rapidly changed when the cold air passes over the relatively warm water of Hudson Bay. The changes in lapse rate, temperature and moisture content are illustrated by the radiosonde observation plotted on the tephigram in Fig. 2. In the diagram, the Churchill sounding (24 November 1947) is compared with the Port Harrison sounding (25 November) taken in the air after modification by Hudson Bay. The flux of heat and rapid evaporation quickly increase the temperature and moisture content of the air. Strong vertical currents produced by the extreme instability extend the modification through a great depth of air (to 720 mb, in Fig. 2).

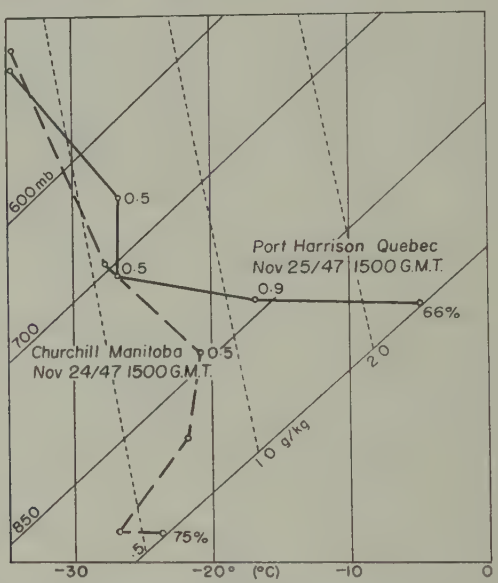

Figure 2. Changes in the vertical structure of cP air crossing Hudson Bay in early winter.

On a tephigram plot of the sounding taken in the modified air a sharp inflection normally occurs between the unstable lapse rate of the modified air and the stable lapse rate of the air aloft. This inflection level was considered as the height to which the modification extended.

The maps and upper-air data for the years 1945 to 1948 were reviewed and the height of modification was computed for suitable cases of westerly airstreams crossing Hudson Bay. The results are shown in Table VI.

The grouping of the height of modification in Table VI into parts of the early winter season indicates several points. The cases in the first half of November have mainly lower heights of modification than the cases in the last half of November. The height of modification decreases in December, and in the last half of December extends to only low levels. The temperature decreases during the fall and early winter and the greater the contrast between the air temperature and the temperature

TABLE VI. The height of modification over Hudson Bay in early winter

Period	Range of values (ft)	Average height of modification (ft)
Nov. 1 to 15	5,000 to 7,500	5,500
Nov. 15 to 30	5,000 to 10,000	8,000
Dec. 1 to 15	2,000 to 8,800	5,000
Dec. 15 to 30	1,000 to 4,800	2,000
Jan. 1 to 30	nil to 3,000	1,200

of Hudson Bay, the greater is the instability and the greater the height of modification. Thus, the modification extends to greater heights in the last half of November than in the first half. This effect might continue into December and January since the colder air masses could then give greater contrast. But this does not happen ; the height of modification actually decreases and is to be attributed to the ice forming on Hudson Bay and decreasing the amount of water surface available for modification.

Another point indicated in Table VI is the great range of values during the month of December. The results on the depth of modification for the December cases were erratic. They varied with the year, indicating that the amount of water available for modification during this month varies greatly from year to year. Most of the cases of wide variation occurred during the first half of December when the ice is forming rapidly.

Another factor that caused variation in the height of modification during the early winter was the lapse rate of the original air. Air streams with deep inversions and stable lapse rates aloft had lower heights of modification. Over the relatively warm water marked instability developed but did not extend very high since air of equivalent temperature was quickly reached at a low level. These cases had anticyclones dominating the circulation over Hudson Bay. Steeper lapse rates (although still with surface inversions) were associated with cyclonic circulations. In such cases the instability produced by Hudson Bay extended to higher levels. When this study was first begun, it was expected that a correlation could be obtained between vorticity and the height of modification. The formation of ice, however, overshadowed the minor changes due to dynamic effects and only the general relation to curvature could be obtained.

(*b*) *Midwinter*

The main feature of the results in the months January to May was the lack of modification. The ice surface of Hudson Bay effectively insulated the cold air from the relatively warm water and so prevented any extensive modification. The results varied from no modification to a few cases of 3,000 ft.

There was a noticeable grouping of cases according to trajectory. Cases with appreciable modification were in very cold north-west circulations while cases with no modification were in south-west circulations. The transmission of heat through the insulating ice surface may account for some of the modification that does occur but this is believed to be a minor factor in comparison to the exposed water surface in the holes and cracks in the ice.

(c) *Summer*

The stabilization of the warm air by contact with the cold water surface confined the lapse-rate changes to the surface levels. A pronounced but shallow marine stratum formed in south-west circulations of warm returning continental polar air. Stratus cloud, fog and drizzle commonly occurred. In cases where no cloud formed the cooling effect of the Bay was rapidly cancelled when the air encountered the warm land. At Port Harrison fog occurred on 56 per cent. of the days with westerly winds in contrast to only 20 per cent. for days with easterly winds. Cool air from the Arctic in cases of north-west circulations showed little modification.

6. Conclusions

In a normal year ice forms in Hudson Bay in November with a rapid increase in the amount and depth during December. From January to April the Bay is covered with pack ice. The ice begins to melt in May and decreases rapidly in June. Some ice fields are present in July with a few remains of ice occasionally present in August. It is estimated that in a year of abnormal temperature there would be a one-month lag or advance from the times given. An unusually mild period during midwinter would cause only a slight temporary decrease in the amount of ice.

During November and December the flux of heat and rapid evaporation from the relatively warm water greatly modify the climate of the surrounding area. Extensive cloud and excessive snowfall result. The temperature though greatly modified is normally below freezing.

During July and August the cold water of Hudson Bay causes sub-normal temperatures and the frequent formation of stratus cloud, drizzle and fog.

Except for these two brief periods (positive modification in early winter and negative modification in midsummer), Hudson Bay has little effect on continental polar air masses. The formation of ice reduces the modification to negligible amounts during midwinter. In the later summer and fall there is little net modification because there is little difference between the water temperature and the mean air temperature.

In the spring and summer the ice and cold water of Hudson Bay act as a source of cold air and allow cold air from the Arctic to sweep down unmodified into northern sections of Ontario and Quebec. In effect, Quebec and Ontario have an Arctic Ocean along their northern boundary.

In midwinter the frozen surface of Hudson Bay makes all of northern Canada a uniform snow and ice surface so that the whole area is a source region for continental polar air masses.

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A study of the transfer of heat and water vapour above a surface of short grass

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SUMMARY

The profiles of temperature, humidity and wind have been determined by conventional methods the lowest 2 m of the atmosphere above a close-cut grass surface, with simultaneous observations the heat absorbed in the soil and of the short-wave and long-wave radiative flux of heat. Twenty-ne sets of observations were made ; the details are given in Appendix I.

The ratio of the gradients of temperature and humidity is found to be independent of height, d changes with height of the vertical convective flux of heat and of water vapour are relatively nall ; the ratio of the coefficients of eddy diffusion of heat and water vapour is therefore independent height. These coefficients are then assumed equal and the convective fluxes of heat and of water pour determined (Bowen's method). The fluxes are also determined by an aerodynamic method nich assumes equality of the eddy viscosity and the appropriate diffusion coefficient. The results the two methods agree in most cases, in both stable and unstable stratifications, but there are some ceptions which cannot be explained.

Absorption of radiation by the air is investigated. It is shown that this is comparable in effect th convection in the control of temperature and relative humidity. Particular attention is paid to ur occasions with very light wind (including two with fog formation) when horizontal mixing peared to be of major importance.

Some examples are presented of wind profiles which do not conform to the law proposed by eacon. A value between 0·5 and 1·5 is suggested for the critical Richardson number.

1. INTRODUCTION

For many years the main stimulus to study of the meteorology of the lowest w metres of the atmosphere was its importance in the problems of chemical war-re, examples being the investigations of Best (1935) and of Deacon (1948, 1949). lore recently the agricultural applications – estimation of evaporation and pre-ction of frost – have been emphasized, and so much attention has been paid to the ıbject that it seems advisable to mention its wider meteorological significance. he surface is the seat of by far the largest transformation of solar energy, and most all the heat, sensible and latent, which drives the atmospheric engine passes rough the surface layers of air. It is clear that detailed observation in these layers ay yield results of interest in the widest meteorological problems. The question nder investigation is ' what happens to the energy of sunlight ? ' ; the observations be described attempt to answer this question for a few specific occasions at one ace.

Within the atmosphere heat may be transferred by conduction, convection, diation, and in latent form by the diffusion and convective transfer of water vapour ; nduction and diffusion are normally negligible except in immediate proximity the surface. At the surface, conduction to and from the lower layers of the soil ust also be considered. Convection may be free, *i.e.* due purely to buoyancy

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forces initially set up in air heated by radiation and conduction, or forced, the air being driven in turbulent motion by external forces. The two types have been distinguished in some meteorological literature as ' convective turbulence ' and ' frictional turbulence.' This distinction will not be made here, nor will any attempt be made to distinguish between conduction and convection at the surface.

It will become clear that our investigation was generally similar to that of Pasquill (1949c) ; there were however certain differences in purpose, method, and results, which appear of some importance.

2. The observations

(a) *Site and duration*

The observations were made at Kew Observatory, on a surface of level grass which was kept mown to an average length of 1 to 2 cm. This was surrounded by a solid fence about 1·5 m high, which was about 50 m away from the site at its nearest point. In certain directions there were huts, buildings and very large trees at distances of 50 to 100 m ; in almost all the experiments the wind blew from directions clear of these obstacles. It is usually accepted that reproducible profiles representative of a given surface can be obtained up to a height of about 1/100 of the distance of the unobstructed flow over the surface ; we therefore expected our results to be of general interest at heights up to about 50 cm. The surroundings of the Observatory, to distances of about 1 km, are level and predominantly of short grass, but are much more broken up by trees, *etc.*, than has been usual at sites previously used for similar work.

The time over which mean values should be taken has long been recognized as a source of difficulty in work of this type. We wished to exclude the effects of diurnal variations from our means, and chose 30 min as probably the maximum time allowable. It proved possible to take sufficient readings in this time to give smooth mean profiles, but it did not seem advisable to reduce the number of observations much further. A reduction of the overall time might have been advantageous, but would have required more observers or an elaborate system of recording instruments. The times of observation given in this paper are the mid-points of the 30-minute runs, unless otherwise stated.

(b) *Air temperature and humidity*

These were measured by the dry and wet bulbs of an installation of aspirated thermocouples similar to that described by Pasquill (1949a), the 'bulbs' being at heights of about 4, 8, 12, 25, 50, 100, and 200 cm above the surface. The linear dimensions of bulbs and housings were about half those used by Pasquill, and with the ventilation rate employed, about 3 m sec^{-1}, the lag coefficient of the dry bulbs was about 65 sec. The standard junction was attached to a copper block similar to the thermometer bulb, and immersed in a Dewar flask which contained paraffin and a heavy copper coil to facilitate heat exchange. It was placed in contact with the bulb of the reference thermometer which had an open scale calibrated to 0·2°F, and read to 0·01°F, readings being taken at frequent intervals in the course of each observation.

The main purpose of the temperature readings was the determination of mean vertical temperature and humidity gradients, secondary considerations being the variation with time and the absolute values of the temperatures and humidities. The equipment chosen is obviously not ideal for this purpose, but any more direct method of determining the gradient (*e.g.* by freely exposed thermocouples at equal small height intervals) would require a major research to establish its validity. It is by no means obvious that the air disturbance caused by ventilation of the thermometers used would be without effect on the measured values, but Best (1935) and Pasquill (1949b, 1949c) had obtained very consistent results with similar apparatus, and in the event we confirmed their experience. A particularly convincing example of this internal consistency occurs in the present results. Profiles taken in very light winds indicate local radiative equilibrium, and whilst there is no direct proof that such equilibrium exists it is most unlikely that this special temperature distribution should be the chance result of faulty temperature measurements. It must however be borne in mind that there is no direct external evidence that the method gives ' true ' temperature values, particularly at the lowest heights, in all circumstances.

Fifteen values of the dry-bulb temperature and wet-bulb depression were obtained in each 30-minutes run, and the means taken. Graphical interpolation was necessary in determining the humidity from temperatures expressed nominally to 0·01°F. Marvin's (1912) tables were used as a basis, being the only tables for aspirated Fahrenheit thermometers available to us. We apologize for the use of both Centigrade and Fahrenheit scales. Supply difficulties made it necessary to use a Fahrenheit standard for the thermocouples and a Centigrade reference for the radiometer, and it was thought advisable to defer conversion until the final stage of analysis.

(c) *The flux of heat in the ground*

The instrument used for earth temperature measurement was similar to that employed by Pasquill (1949c), in which a number of copper strips were set on an ebonite rod which was pushed into the ground. In the first model six strips were used, at depths of 1, 2, 4, 8, 16 and 32 cm below the surface, a later design had additional strips at 3 and 6 cm. Three sets of readings were taken during each observation, before, during, and after the air temperature measurements. It will be seen later that uncertainties in the heat transfer to and from the ground, though relatively large, were small in comparison with those of measurements of the radiative flux, and had only a secondary effect on the overall accuracy of the work.

The nature of the soil at Kew may be judged from the following figures. The density after drying for several hours at 105°C was about 0·3 g cm^{-3} for the top two centimetres, then about 1·1 g cm^{-3} down to at least 30 cm. Drying at this temperature after a prolonged rainless spell expelled about 10 per cent. by weight of water ; when saturated (but not muddy) after rain about 45 per cent. by weight was expelled. The specific heat of the oven-dried material was $0{\cdot}30 \pm 0{\cdot}02$ cal $°C^{-1}$ g^{-1}, with no significant variation with depth. A number of determinations of the thermal conductivity of the top layers of soil in the naturally dry condition gave results between 2×10^{-4} and 5×10^{-4} cal cm^{-1} sec^{-1} $°C^{-1}$, with a mean of $3{\cdot}2 \times 10^{-4}$.

(d) *The radiative flux of heat*

The flux of ' short-wave ' radiation was measured by a Moll solarimeter mounted horizontally near the site, its output being recorded continuously. From time to time during each observation the instrument was inverted to give a measurement of the flux reflected from the grass. This instrument was supplemented by two similar thermopiles mounted on the Observatory roof.

The solarimeters were standardised by shading the thermopiles from direct solar radiation by a small disc and simultaneously measuring the solar intensity by means of a standard Ångström pyrheliometer (No. 24). It was not possible to perform a standardization during the course of each experiment, and our experience with the solarimeters suggests that the results may be in error by as much as 10 per cent, this being the largest source of error in an observation. This view of the accuracy obtainable from these instruments, less optimistic than that of Pasquill (1949c), appears to be confirmed by Nicolet (1948).

The long-wave radiation of atmospheric or terrestrial origin was measured by means of a Linke-Feussner radiometer directed alternately on sky and ground at an angle of 52·5° to the vertical, the surface temperature being checked directly by a grass thermometer (Robinson 1947, 1950a ; these papers also contain details of the standardization of the radiometer). In most of the experiments cloud amounts were negligible, but one run (16 June 1949, 1620 GMT) was made with 10/10 fairly uniform cloud, and during two others (15 June 1949, 2130 GMT and 17 June 1949, 2133 GMT) up to 6/10 of thin medium cloud was present. In these circumstances an attempt was made to examine a representative sample of the sky when using the radiometer. In daylight it was possible to take three or four radiometer determinations each on ground and sky during the course of an experiment, at night six to eight determinations were made on each. The probable errors of this measurement, even in the worst cases of partly clouded skies, were less than those of the short-wave radiative flux.

(e) *The change of temperature and humidity with time*

To compute the changes with height in the fluxes of heat and water vapour it was necessary to know the time rate of change of temperature and humidity. For each experiment the individual readings at each height were plotted against time, and a smoothed curve drawn through them. The mean rates of change over the experimental periods were computed from the end points of the smoothed curves rather than from the first and last readings, in order to minimise the effects of the fluctuations.

(f) *Wind*

Wind measurements were made with sensitive cup anemometers similar to that described by Sheppard (1940). The number available varied, and in one case no wind profile was obtained, but for most of the observations eight anemometers were exposed at heights (mid-point of cups) of about 5, 12, 25, 37, 50, 75, 100 and 200 cm. For each of the two lowest heights it was necessary to sink the dial of the

nemometer below the surface, and to work the control by means of a spring and ord. Some disturbance of the surface was entailed, but this was well within the ea swept by the cups. Because of lack of observers the anemometers were left record for the 30 min of each run, providing a mean wind for the whole period.

Anemometers of the type used measure the run of the wind, not its component a given direction, and will respond to the transverse velocities of eddies with imensions larger than the circle swept by the cups. It is, however, customary to lentify their indications with the ' magnitude of the vector mean wind ' of theoretical eatments. The point is mentioned because the present experiments contain a umber of occasions when large variations of wind direction were observed on the te. Another point of interest is connected with the stopping of the cups in very ght winds of about 15 cm sec^{-1} or less. This was seen to occur during one of the uns, and probably did so during at least two others. No correction to these esults was possible, but they were not rejected. It can be seen that the effect would robably lead to an under-estimation of wind speed greater for lighter winds, *i.e.* or lower heights, and hence to an over-estimation of wind gradient of magnitude ecreasing with increasing height.

3. Method of analysis of the results

(a) *Notation*

The following symbols, units, and conventions will be used :

z = Height above surface (cm).

T = Temperature (°C unless otherwise stated).

e = Water vapour concentration (g per g of air).

u = Wind speed (cm sec^{-1}).

$F(z)$ = Total vertical flux of heat at height z (cal cm^{-2} sec^{-1}).

$C(z)$ = Non-radiative flux of heat at height z (cal cm^{-2} sec^{-1}).

$R(z)$ = Radiative flux of heat at height z (cal cm^{-2} sec^{-1}).

$G(0)$ = Flux of heat in ground at the surface (cal cm^{-2} sec^{-1}).

$E(z)$ = Vertical flux of water vapour at height z (g cm^{-2} sec^{-1}).

$M(z)$ = Vertical flux of momentum at height z (g cm^{-1} sec^{-2}).

$\tau(0)$ = Shearing stress at the surface (g cm^{-1} sec^{-2}).

$K(z)$ = Eddy conductivity, defined by $-C(z)/\rho c_p \left(\frac{\partial T}{\partial z} + \Gamma\right)_z$ (cm^2 sec^{-1}).

$D(z)$ = Eddy diffusivity of water vapour, defined by $-E(z)/\rho \left(\frac{\partial e}{\partial z}\right)_z$ (cm^2 sec^{-1}).

$N(z)$ = Eddy viscosity, defined by $-M(z)/\rho \left(\frac{\partial u}{\partial z}\right)_z$ (cm^2 sec^{-1}).

k, d, ν = Molecular thermal diffusivity, diffusivity of water vapour and kinematic viscosity respectively ($cm^2\ sec^{-1}$).

$A(z) = D(z)/K(z)$.

$$f(z) = \frac{\partial e}{\partial z} \Big/ \left(\frac{\partial T}{\partial z} + \Gamma\right).$$

Γ = The dry adiabatic lapse rate $= -1 \times 10^{-4}$ °C cm^{-1}.

$$Ri = \text{Richardson's number} = g\left(\frac{\partial T}{\partial z} + \Gamma\right) \Big/ T\left(\frac{\partial u}{\partial z}\right)^2.$$

Fluxes directed away from the surface are considered positive. In practically every instance to be discussed the temperature gradient is numerically much greater than Γ, and $\partial T/\partial z$ is used to replace $(\partial T/\partial z + \Gamma)$ with negligible error.

In the numerical work, necessarily very approximate, ρ and c_p, the density and specific heat at constant pressure of air, are taken as $1{\cdot}2 \times 10^{-3}$ g cm^{-3} and 0·25 cal $°C^{-1}$ g^{-1} respectively. For latent heat of condensation of water, λ, the value 600 cal g^{-1} is used.

(b) *The relation of convective flux and gradient – advection*

The definitions which we have adopted of eddy conductivity and eddy diffusivity must not be allowed to obscure a fundamental weakness of this type of investigation. These definitions are in terms of the ratio of the appropriate flux to its corresponding vertical gradient, but since this ratio is known to be variable over a very wide range of values, according to position and meteorological conditions, it is obvious that they are not based on direct experimental evidence. They originated by analogy with molecular conduction and diffusion, and have been supported by a derivation based on mixing-length theory (*e.g.*, Brunt 1939). In this derivation, in addition to the artificial mixing-length hypothesis, three assumptions are made.

(i) There is no correlation between the subsequent vertical motion of a parcel of air instantaneously at rest in the vertical and its excess of the property considered over the mean value at the level at which it is at rest.

(ii) The appropriate vertical gradient is linear over the range of vertical motion of individual parcels of air.

(iii) The appropriate horizontal gradient is negligible.

Of these, (i), at least in the case of transfer of heat, may be inconsistent with the existence of free convection, (ii) is questionable on several counts (compare, *e.g.*, with the definition of v. Karman's constant in terms of the ratio of the first and second derivatives) ; and whilst (iii) is not a fundamental objection in theory, it raises a number of practical difficulties. If the horizontal gradient is not negligible a certain proportion of the convective flux will be associated with it, and in investigations in which observations are made in one position only it is not possible to separate this advective term.

Advection may be on a large scale, detectable on a synoptic chart, or on a small scale, due to local inhomogeneity of terrain. The advection term is most easily

dealt with in the case of momentum transfer, where small-scale advection has been to some extent studied in the search for wind profiles representative of different surfaces. It is now generally accepted that the wind profile over a uniform surface is effectively independent of surroundings up to a height of about 1/100 of the fetch of wind over the surface. This was suggested by wind-tunnel experience, and has been confirmed by the internal consistency of a long series of wind profile observations culminating in those of Deacon (1948, 1949). It is clear that elimination of advection of momentum does not necessarily imply no advection of heat or of water vapour. On the large scale, advection of heat is recognized as a major climatic factor, whilst on the small scale a surface of uniform roughness may be obviously inhomogeneous in thermal properties or dampness. The difficulty is inherent in all methods of estimation of convective exchange of heat and water which, like the present, employ the eddy coefficients, and it must be kept in mind when the results are considered.

(c) *Determination of the fluxes by the heat-balance method. Bowen's ratio*

Conservation of energy in the vanishingly thin surface layer gives

$$C(0) + \lambda E(0) + R(0) + G(0) = 0 \quad . \quad . \quad . \quad (1)$$

Thus the convective loss of sensible and latent heat, $C(0) + \lambda E(0)$, is determined by the observations, but to separate the terms a new relation is required. The usual method of establishing such a relation follows that of Bowen (1926) who determined $C(0)/\lambda E(0)$, often called Bowen's ratio, from measurements of temperature and humidity at two heights. The conception of eddy diffusion is not explicit in Bowen's original paper, but subsequent modifications of his method have been made (*e.g.*, Sumner 1947) in which it is found necessary to assume equality of K and D. The following treatment is a simple variant of the method. We have

$$\lambda E(z)/C(z) \equiv \lambda/c_p \,.\, A(z)f(z) \quad . \quad . \quad . \quad . \quad (2)$$

$$E(z) - E(0) = -\int_0^z \rho \, \partial e/\partial t \, dz \quad . \quad . \quad . \quad . \quad (3)$$

$$C(z) - C(0) = -(R(z) - R(0)) - \int_0^z \rho \, c_p \, \partial T/\partial t \, dz \quad . \quad (4)$$

It will be shown from the observations, Section 4 (c), that the term on the right of Eq. (3) is always small, of the order 1 per cent. of $E(0)$ up to 1 m, and that the term on the right of Eq. (4) involving dT/dt is of similar order. The other term on the right of Eq. (4) comprises the change of radiative flux between the surface and the height considered. This term is measurable in principle, or may be calculated, *e.g.*, by means of a radiation chart, if temperatures and humidities are known. Trials using the Elsasser chart (Elsasser 1942) or the Kew chart described by Robinson (1950a) indicated that the temperature and humidity results up to 2 m were sufficient to allow fair accuracy in computing the flux changes near the ground. These changes were found to be often of the order of 10 to 100 times the total heat-flux change computed from the increase or decrease of temperature, but probably not

more than 10 per cent of $C(0)$ in the lowest metre. With an error of up to about 10 per cent we therefore write Eq. (2)

$$\lambda E(0)/C(0) \simeq \lambda/c_p \, . \, A(z)f(z) \quad . \quad . \quad . \quad . \quad (5)$$

The observations also show (Section 4(b))

$$f(z) = \text{constant}$$

for each observation, within similar limits of error, which, together with Eq. (5) implies

$$A(z) = \text{constant}$$

i.e., the ratio of eddy diffusivity and eddy conductivity does not vary with height. The ratio A cannot be determined by this method, for although an equation involving A and quantities theoretically measurable in certain conditions can be set up, these quantities include terms in $\partial^2 T/\partial z^2$ and $\partial^2 e/\partial z^2$ and it is not possible to obtain the profiles with anything approaching the precision required for a reliable determination of the second derivative.

We are therefore compelled to fall back on the assumption made by most users of Bowen's method, namely $K = D$, *i.e.*, $A = 1$. Since we have shown A constant with height it is indeed difficult to explain any value other than unity, which has the support of the mixing-length theory. Emmons and Montgomery (1947) have pointed out the difficulties that arise when considering transition between turbulent and molecular diffusion owing to the non-equality of the molecular-diffusion coefficients k and d, and a case might be made out for the assumption $A = d/k$, *i.e.*, about 1·2 at surface-layer temperatures and pressures*. The accuracy of the present measurements is however not sufficiently great to make the distinction between $A = 1$ and $A = 1{\cdot}2$ significant, and the former value will be adopted. We therefore write

$$\lambda E(0)/C(0) \simeq \lambda/c_p f \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which, with Eq. (2), allows approximate computation of the fluxes and eddy coefficients.

(d) *Determination of the fluxes by the aerodynamic method*

The variation of mean wind speed near a rough boundary surface has been thoroughly investigated, and a profile law of the form

$$u = 1/k \, (\tau(0)/\rho)^{\frac{1}{2}} \log (z + z_0)/z_0 \quad . \quad . \quad . \quad (7)$$

established empirically, z_0 being a constant, with dimensions of length, associated with the nature of the surface, and k (v. Karman's constant) having the value 0·40 in a wide variety of circumstances. Sheppard (1947) working over an artificial surface, confirmed that this law is valid for atmospheric flow if the lapse rate is near the adiabatic value, and Pasquill (1950) has extended the confirmation, with

* Pasquill (1942, see p. 88) in studying the evaporation of a number of liquids has advanced empirical evidence that the eddy coefficients are functions of the corresponding molecular coefficients. It is also noteworthy that although Bowen in his original paper nowhere speaks of eddy diffusion; his work is most readily interpreted on these lines, and his numerical values imply the assumption $A = d/k$.

minor modifications, to natural grass surfaces. We shall regard it as an empirically established law. If we identify $\tau(0)$ with $M(z)$ in the steady state (which is equivalent to assuming local conservation of momentum in the turbulent flow, but which has not been experimentally established), Eq. (7) leads to

$$N(z) = k^2 z^2 \, \partial u/\partial z \qquad . \quad . \quad . \quad . \quad . \quad (8)$$

Deacon (1949) has shown that Eq. (7) is only valid in neutral stability. A convenient index of stability is provided by Richardson's number, which is zero for neutral stability, negative in unstable, and positive in stable conditions. Deacon's experiments (and the present results at smaller heights) show that Ri usually becomes small as the surface is approached, and it seems clear, as pointed out by Deacon, that, at least over short grass, Eq. (7) provides a close approximation to the profile near the boundary in practically all stabilities, and that Eq. (8) holds generally as a good approximation at small values of z.

There is thus an empirically established method for determination of the eddy viscosity from the wind profile alone, without the need for simultaneous determination of a momentum flux or shear. If now we equate eddy viscosity to eddy conductivity or diffusivity, we may estimate the convective loss of sensible or latent heat from simultaneous measurements of the profiles of wind and of temperature or humidity, providing the measurements are made sufficiently close to the ground. It has been customary to regard the eddy viscosity, diffusivity, and conductivity as equal, but the only theoretical justification has been in the rather vague ideas of the mixing-length theory. If however the use of this (the aerodynamic) method of estimating evaporation and convective heat transfer is found to give the same results as direct measurement, or as the ' energy balance ' method using Bowen's ratio, the equality of the coefficients may be regarded as established in the conditions of the experiment. We shall see that the present results indicate approximate equality of the fluxes determined by the two methods, implying approximate equality, at least in many circumstances, of the eddy coefficients.

4. Examination of the results – computation of the convective fluxes

(a) *Are the profiles representative of the surface?*

As remarked in Section 2(a) it was expected that representative profiles would be obtained up to about 50 cm. It is obvious that this could not be confirmed directly since we were not prepared to make any assumptions concerning the form even of a representative profile. However it was found that discontinuities of the wind profile were rather common between the 25 cm and 37 cm anemometers, though sometimes they were noted only at greater heights. Examples of this are shown in Fig. 1, in which the wind profiles of 12 Sept. 1949, 2200 hr and 28 Sept. 1949, 1905 hr are plotted on a logarithmic height scale, and others may be seen in some of the profiles in Fig. 2. The effect was much less obvious on the temperature profiles, but was probably present to a slight degree in some cases, and was very marked on one occasion. It seems reasonable to suggest that these discontinuities mark the top of the region of horizontal homogeneity of wind velocity.

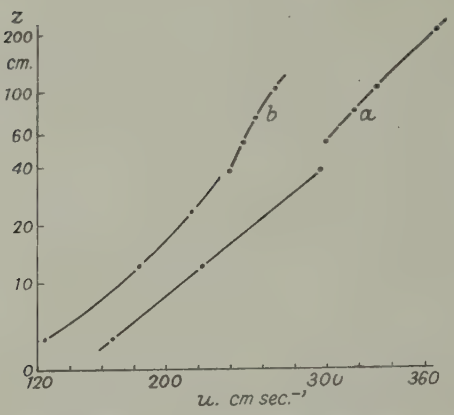


Figure 1. Examples of wind profile. (a) 12 Sept. 49, 2200 GMT. (b) 28 Sept. 49, 1805 GMT.

(b) *Determination of the gradients and the ratio f*

The choice of method of deducing the gradients from the observations is probably the most important step in the analysis of the results. It was soon apparent that although smooth curves could usually be drawn through the observed temperatures it was very difficult to construct humidity profiles at all satisfying as unique representations. Pasquill (1949c) avoided this difficulty by assuming a definite functional form for the profiles, the same for both temperature and humidity. This included three ' constants ' one of which was allowed to differ in different height ranges. We were reluctant to base conclusions regarding the ratio of the gradients of temperature and humidity at different heights on a method which assumed the same functional form for the profiles, and which at the same time obscured the considerable observational scatter of the humidity points. It was therefore decided to draw (without mechanical aid) smooth curves through the temperature and humidity points, and to obtain the gradients by tangent drawing at the points of observation. In this process a number of humidity points were rejected, in circumstances which made it almost certain that a large systematic error was present. (The criterion of rejection was that the observed humidity should be higher than that suggested by the other points by an amount at least four times that of the deviation of any other point from the curve. Practically all the rejected points were from either the 100 cm or the 4 cm wet bulb, and were over a consecutive series of observations. They are thought to have been due to the wet bulb fouling the housing, or to faulty insulation of the two thermocouple leads in the stem of the thermometer). Even after rejection of these points many of the humidity profiles looked by no means convincing, but it is doubtful whether more careful work with existing methods can produce really definite humidity profiles, since in most of our observations the deviation of the observed point from the curve drawn was less than 1 per cent of the concentration, and few of the deviations were more than 2 per cent. In contrast to this scatter of the humidity observations, the smoothness of the temperature profiles was most surprising. Although based on 15 (or less) readings of a fluctuating quantity, the deviations from a smooth curve were only of the order of 1 per cent of the fluctuations.

TABLE I. MEAN VALUES OF THE MAGNITUDE OF f FOR ALL OCCASIONS

$\lvert f \rvert$ By tangent drawing					$\lvert f \rvert$ by curve superposition
$z = 7{\cdot}5$ cm	$z = 13{\cdot}5$ cm	$z = 25$ cm	$z = 50$ cm	$z = 100$ cm	
4·80	4·19	4·30	4·98	5·31	4·33

The gradients obtained by tangent drawing were used to compute values of at each height for each observation, and Table I shows the means of the magnitude of f for all observations taken between 19 July 1948 and 1 July 1949. No physical significance is attached to the means, but they should eliminate to some extent errors due to uncertainties in the drawing of individual profiles and tangents. Remembering that the profiles were probably often not representative above 30 cm, and that there was much uncertainty in deriving gradients by tangent drawing at 7·5 and 100 cm, it seems reasonable to conclude from Table I that f is constant with height. A more convincing demonstration was however obtained by superposition

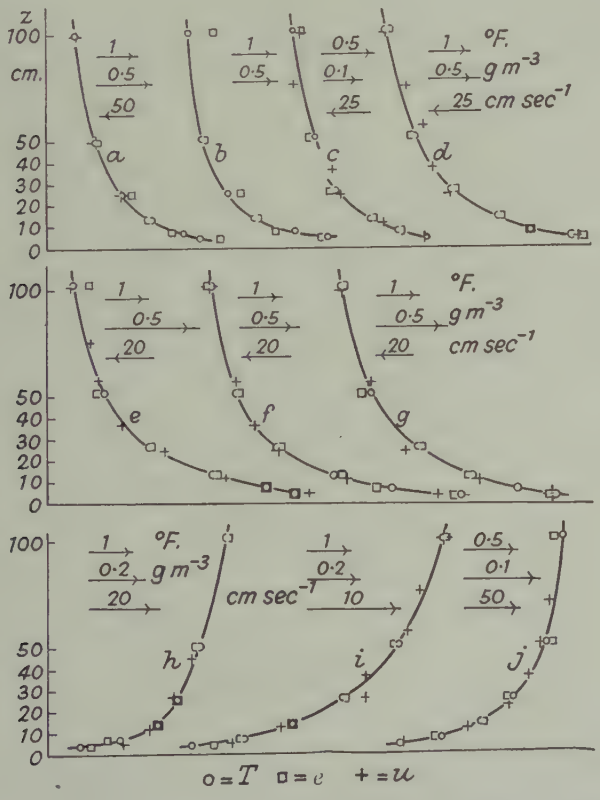


Figure 2. Examples of superposed profiles of temperature, humidity and wind. (a) 1 July 48, 1415 GMT. (b) 2 Oct. 48, 1110 GMT. (c) 16 June 49, 1620 GMT. (d) 20 June 49, 1410 GMT. (e) 21 June 49, 1400 GMT. (f) 24 June 49, 1419 GMT. (g) 1 July 49, 1122 GMT. (h) 10 Nov. 48, 1750 GMT. (i) 26 Nov. 48, 1610 GMT. (j) 28 Sept. 49, 1905 GMT. The 200 cm observations, which were used to assist superposition, have been omitted to economize space.

of the temperature and humidity profiles. The scales were adjusted so that the profiles drawn by hand coincided at 10 cm and 100 cm, and temperature and humidity points were plotted on one curve using the appropriate scales. Some of these superposed curves are shown in Fig. 2 (other examples have already been published by Robinson (1950b)), and the mean magnitude of f derived from the ratios of the scales is given in Table I ; the individual values comprise Col. 10 Table II.

The curves of Fig. 2 are the most direct evidence available in the present results that the temperature and humidity profiles have the same form ; the most doubtful occasion is 2 Oct. 1948, 1110 hr, but even here the deviations of the humidity observations are within the estimated range of experimental error.

In Fig. 2 the wind profiles have been fitted to the temperature profiles in the same way, by superposition at 10 cm and 100 cm and adjustment of the scales. The indications are that the wind and temperature profiles frequently have the same or nearly the same form, but that significant deviations may occur on some occasions.

(c) *The heat balance method*

Table II contains details of the measurements and computations of the terms in Eqs. (1), (3), (4), and (6), with the fluxes computed using Eqs. (1) and (6). It is necessary to justify the remaining assumption made in deriving Eq. (6) by appeal to these observations. It is required to show that $E(z)/C(z)$ may be replaced by $E(0)/C(0)$ with reasonable accuracy. Table II shows that $C(z) - C(0) + \lambda\{E(z) - E(0)\}$ is small compared with $C(0) + \lambda E(0)$, but, in consequence of radiative transfer, $C(z) - C(0)$ is normally much greater than $\lambda E(z) - \lambda E(0)$. To get some idea of the error involved in neglecting $C(z) - C(0)$ in comparison with $C(0)$ we must use a trial value for the ratio $\lambda E(0)/C(0)$. The term $C(z) - C(0)$ is relatively great when variation in $R(z)$ is great, *i.e.*, when surface temperature and air temperature differ widely. As these are the circumstances in which $C(0)$ would not be expected to be small we try first the assumption $\lambda E(0)/C(0) \not> 3$ (*i.e.*, Bowen's ratio of magnitude not less than 1/3). Substitution of this value in the cases of large $C(z) - C(0)$ gives a variation of E with height in the first metre of about 1 per cent, and a variation of C of about 20 per cent. With an assumed value 1 for Bowen's ratio the greatest variation of C is about 10 per cent up to 1 m. It is therefore reasonable to suggest that Eq. (6) holds for all the cases investigated, and that its use introduces errors of not more than 20 per cent.

It is convenient here to recapitulate the steps used in the derivation of the fluxes, and to separate observation and assumption. It is observed that f is constant with height, at least to within a few parts per cent. It is observed that $C(100) - C(0) + \lambda\{E(100) - E(0)\}$ is less than 5 per cent of $C(0) + \lambda E(0)$ in all cases. It is then assumed that $C(0) + \lambda E(0)$ is not overwhelmingly composed of one or other of its component terms, in particular that when $C(100) - C(0)$ is large $\lambda E(0)/C(0) \not> 3$. It follows that, with an error of not more than 20 per cent in the most unfavourable case, the ratio A is constant with height. The further, not unreasonable assumption $A = 1$ is then made. The results of Table II Cols. 11 and 12 follow. We may remark that in many previous applications of the heat-balance method the assumptions of constancy of the fluxes over small height ranges, and of equality of the eddy coefficients, have been made without question.

Date	Time	Downward short-wave flux × 10^4	Reflectivity per cent	Downward long-wave flux × 10^4	Upward long-wave flux × 10^4	$R(0)$ × 10^4	$G(0)$ × 10^4	$C(0) + \lambda E(0)$ × 10^4	f × 10^4	$C(0)$ × 10^4	$E(0)$ × 10^6	$F(100) - F(0)$ × 10^6	$R(100) - R(0)$ × 10^6	$C(100) - C(0)$ × 10^6	$E(100) - E(0)$ × 10^6
1948															
19 July	1415	177	16	80	135	-95 ± 9	$+5 \pm 3$	99 ± 9	6·0	37	9·0	−10	−300	+290	−2·5
2 Oct.	1110	135	22	72**	111	-65 ± 11	$+10 \pm 3$	55 ± 11	6·7	21	5·7	−1	−130	+130	+3
10 Nov.	1510	16	16	72	86	$+1{\cdot}0 \pm 0{\cdot}4$	$+1 \pm 1$	-2 ± 1	††	−2	0	+25	+23	−2	—
10 Nov.	1750	—	—	72·9	78·6	$+5{\cdot}7 \pm 0{\cdot}2$	$-2{\cdot}2 \pm 0{\cdot}7$	$-3{\cdot}5 \pm 0{\cdot}7$	2·2	−2·3	−0·5	+16	+11	−5	+1·7
11 Nov.	1415	22	18**	76	88	$-6 \pm 2{\cdot}5$	$+3 \pm 1$	$+3 \pm 3$	−12·2	−1·5	0·7	−5·5	+1	−6·5	—
*24 Nov.	1755	—	—	58·7	74·7	$+16 \pm 1$	$-0{\cdot}5 \pm 0{\cdot}5$	-16 ± 1	1·4	−12	−0·6	+3	+37	−34	0
*26 Nov.	1610	—	—	62·4	74·8	$+12 \pm 2$	$-2{\cdot}5 \pm 1$	-10 ± 2	2·4	−6·4	−0·6	+15	+15	0	−3
†*26 Nov.	1745	—	—	61·1	73·0	$+12 \pm 2$	$-2{\cdot}5 \pm 0{\cdot}5$	-10 ± 2	2·4	−6	−0·6	+12	+7·5	−5·5	−2
1949															
3 May	1407	163	20·6	70	107	-88 ± 10	$+12 \pm 3$	75 ± 10	6·1	31	7·5	0	−105	105	0
7 June	2125	—	—	71·4	87·8	$+16{\cdot}5 \pm 0{\cdot}5$	-4 ± 1	$-12{\cdot}5 \pm 1$	0·8	−10·5	−0·3	+10	+35	−25	−3·5
15 June	2130	—	—	74	83·5	$+9{\cdot}5 \pm 2{\cdot}5$	-3 ± 1	$-6{\cdot}5 \pm 3$	−1·4	−10	0·5	+20	+40	−20	−2·2
16 June	1620	50	20**	81	100	-21 ± 8	$+3{\cdot}5 \pm 1{\cdot}5$	17 ± 8	8·8	5·5	1·9	−5	−55	−50	0
17 June	2133	—	—	76·5	82·2	$+5{\cdot}7 \pm 2$	$-3{\cdot}5 \pm 0{\cdot}5$	-22 ± 2	0·7	−2	−0·05	+2·5	+26	−24	5
18 June	1121	—	—	—	110	—	—	—	3·4	—	—	+7	−150	160	−0·7
20 June	1121	178	19·5	79	134	-91 ± 13	$+25 \pm 10$	67 ± 17	3·7	34	5·5	−13	−300	290	−2·1
20 June	1410	157	22	82	125	-79 ± 8	$+10 \pm 5$	69 ± 9	5·9	29	6·9	−8	−205	200	−2·5
21 June	1111	193	21·5	77·4	128·5	$+100 \pm 8$	$+10 \pm 5$	90 ± 9	4·5	48	7·7	−10	−250	240	−1
21 June	1400	163	22	86·0	136	-77 ± 8	$+13 \pm 6$	64 ± 10	3·5	34	4·9	−10	−280	270	−1·6
21 June	1611	115	23	84	116	-57 ± 7	$+11 \pm 5$	46 ± 9	4·6	22	4·0	+8	−100	110	−0·5
23 June	1103	198	21·3	79	132	-103 ± 10	$+10 \pm 5$	93 ± 11	2·6	49	5·3	−2·5	−260	260	−0·7
23 June	1432	160	22	77	121	-79 ± 9	$+10 \pm 5$	69 ± 10	2·6	42	4·5	+13	−170	180	−1·5
23 June	1636	102	24·5	77·4	113	-41 ± 7	$+10 \pm 3$	31 ± 8	4·0	16	2·5	+5·8	−115	120	−1
24 June	1419	158	21	78	135	-68 ± 8	$+10 \pm 5$	57 ± 9	5·3	25	5·4	−1·2	−240	240	−1
1 July	1122	198	20·5	82	136	-104 ± 10	$+10 \pm 5$	94 ± 11	4·7	43	8·4	−10	−280	270	+1
1 July	1428	172	20	84	130	-91 ± 9	$+20 \pm 10$	71 ± 13	4·6	33	6·2	+4·2	−180	180	−3·5
5 Sept.	2201	—	—	81·1	93·6	$+12 \pm 1$	$-1{\cdot}5 \pm 0{\cdot}2$	-11 ± 1	—††	−11	0	−1·2	+57	−58	−1
10 Sept.	2128	—	—	84·6	76·1	$+8{\cdot}5 \pm 0{\cdot}3$	-5 ± 1	$-3{\cdot}5 \pm 1$	4·7	2·3	−0·3	+9	+35	−26	−0·2
12 Sept.	2200	—	—	—	—	—	—	—	2·3	—	—	—	—	—	—
28 Sept.	1905	—	—	79	93	$+14 \pm 1{\cdot}5$	$-2{\cdot}5 \pm 0{\cdot}5$	$-11{\cdot}5 \pm 1{\cdot}2$	2·1	−7	−0·7	+2·3	+38	−36	0·4

* Surface temperature below freezing point. Freezing not observed on undisturbed grass, but may have been present, constituting an indeterminate heat source.

† Duration of observations 10 min only. ** Estimated value. †† Humidity almost constant, f indeterminate.

(d) *Comparison with Pasquill's results*

The assumption $K = D$, usually made in this type of work, and partially established by our results, has been questioned by Pasquill (1949c). It is therefore necessary to compare critically his and our methods. Pasquill was able to give definite values to K and D by measuring $E(0)$, together with other quantities in Eq. (2), simultaneously with profile measurements. He finds (see his Fig. 5, p. 136) good agreement between K and D in stable stratifications, but $K > D$ in unstable conditions.

As mentioned in Section 4 (b), Pasquill obtained his gradients by an objective method, and values of f which may be deduced from these gradients show in some cases a variation with height. We found, however, that, if his profiles were superposed, they showed, as did ours, constancy of f within the range of observational error. By Eq. (6) then, Pasquill's measurements are consistent with $K = AD$ at all heights for all his observations. This is inconsistent with his Fig. 5, which suggests a relation between K and D depending on Ri, which itself changes with height. In view of this we do not consider Pasquill's results as invalidating our assumption of $A = 1$, even though his argument is supported by a direct measurement of E.

(e) *The aerodynamic method*

The main practical difficulty in the application of Eq. (8) to the computation of the flux of heat or water vapour by equating the eddy viscosity to eddy conductivity or diffusivity, is to obtain accurate values of the gradients. We have

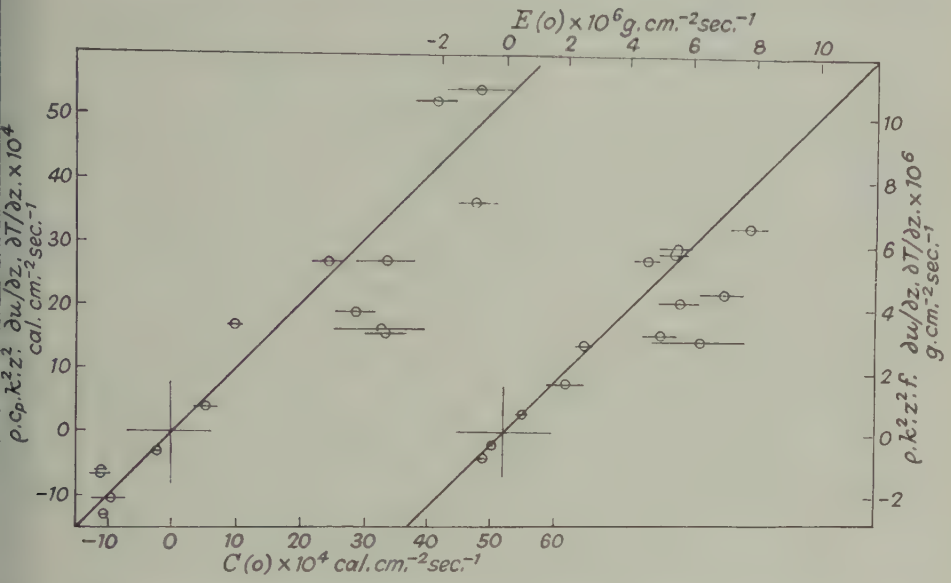
$$C(z) \equiv -\rho c_p K\left(\frac{\partial T}{\partial z} + \Gamma\right) = -\rho c_p k^2 z^2 \frac{\partial u}{\partial z}\left(\frac{\partial T}{\partial z} + \Gamma\right) \quad . \quad (9)$$

and

$$E(z) \equiv -\rho D \frac{\partial e}{\partial z} = -\rho k^2 z^2 \frac{\partial u}{\partial z}\frac{\partial e}{\partial z} \quad . \quad . \quad . \quad . \quad (10)$$

expressions which contain the product of two gradients. Since these expressions are to be applied at the lowest possible values of z, where tangent drawing is least precise, it is necessary to find an alternative method for determination of the gradients.

It was decided to limit discussion to the 15 occasions when the observations allowed confident drawing of smooth curves through the temperature and wind observations below 30 cm, the plotted points lying strictly on the curves. Values were then read from these curves at 2 cm height intervals, and Rutledge's (1932) method of numerical differentiation applied at heights of 10, 15, 20 and 25 cm. This procedure ruled out any direct use of the humidity profiles, only one of which (21 June 1949, 1111 hr), was sufficiently free from scatter, and $f\,\partial T/\partial z$ was substituted for $\partial e/\partial z$ in computing the water-vapour flux. Fig. 3 shows $C(z)$ and $E(z)$ computed in this way for $z = 10$ cm, plotted against the corresponding results of the heat-balance method from Table II Col. 11, the limits of error shown in the latter being estimated from the uncertainties of the measured quantity $C(0) + \lambda E(0)$. The flux at heights above 10 cm was calculated for eleven of the fifteen cases. When this was plotted against height it was found that in most cases extrapolation to



igure 3. Comparison of the fluxes of heat and water vapour computed by heat balance and aerodynamic methods.

$= 0$ improved the agreement with the flux as determined from the heat balance, ut that there were two notable exceptions, 21 June 1949, 1400 hr and 1 July 1949, 428 hr. The computed flux was also plotted against the Richardson number for ie same height, and again it was found that extrapolation to $Ri = 0$ improved ie agreement with the fluxes determined from the heat balance, except on the same vo occasions. The observations did not however conform to any universal relation f the type proposed by Holzman (see Pasquill 1949c, p. 130) ; the constant σ in

$$N = K = D = k^2 z^2 (1 - \sigma Ri)\, \partial u/\partial z$$

aving a different value for each occasion. Fig. 4 illustrates the extrapolation for 1 June 1949, 1111 hr and 21 June 1949, 1428 hr, the former being the most conncing example of extrapolation to the heat balance value of $E(0)$; the latter, one f the obviously exceptional cases.

We conclude from this that the heat-balance method of flux computation, with ie assumption $K = D$, often leads to the same result as the aerodynamic method ith the assumption $K = D = N$, so long as the latter method is used for z small, when extrapolation to $z = 0$ or $Ri = 0$ can be made. There are, however, xceptional cases, and our observations are insufficient to explain these, or even allow an estimate of their frequency of occurrence. They may be connected with e presence of a large advective term, but there is no evidence on this point. The casions on which the two methods agree include both stable and unstable contions, and constitute further indirect evidence against Pasquill's conclusion that is systematically greater than D in unstable conditions.

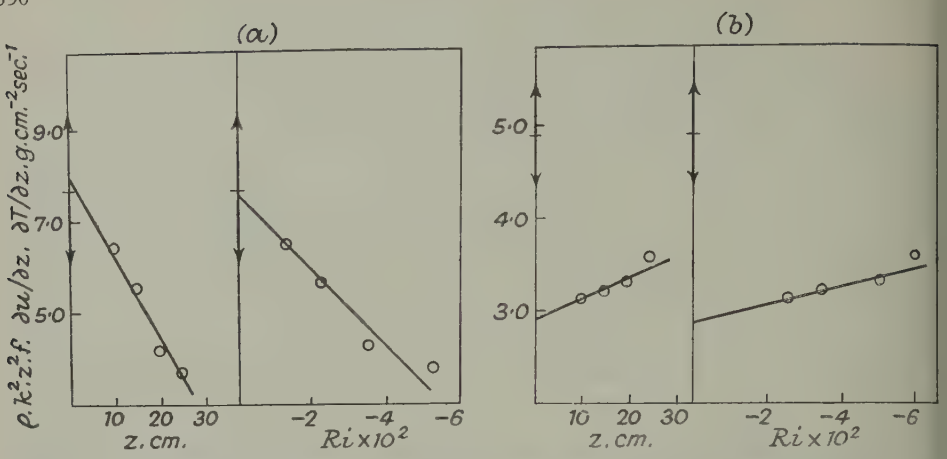


Figure 4. Extrapolation of the flux of water vapour computed by the aerodynamic method to zero height and Richardson number. (a) 21 June 49, 1111 GMT. (b) 21 June 49, 1400 GMT.

6. The variation of temperature and humidity with time

(a) *The fluctuations of temperature*

In previous sections we have been mainly concerned with the magnitude of the various components of the heat flux. We now consider their variation with height, which controls the change with time of temperature and humidity. Fig. 5 shows a number of examples of the variation of individual temperature readings during the course of an observation, and it is apparent that there are fluctuations of temperature of magnitude quite comparable with the temperature change in 30 min. The existence of a convective flux of heat implies temperature fluctuations ; the flux through any level may be written

$$C(z) = c_p \overline{\rho\, t' w'}$$

where t' is the instantaneous departure from mean temperature at a point in the level, and w' the simultaneous vertical component of wind at the point. Very near the ground, however, in a horizontally homogeneous layer, it can readily be seen from a study of the temperature profiles that if t' is of order 1°C or more, the fluctuations, which can only be caused by vertical motion, must be of very short period. It was therefore expected that the relatively large lag of the thermometers used in the present experiments would reduce the observed fluctuations to very small values. Temperature variations of the observed amplitude, with ' periods ' of order 1 min or more, at these heights, cannot be caused by vertical movements of air if the mean temperatures are horizontally homogeneous and the radiative fluxes constant in time. The only explanation which can be offered is that the mean temperatures are not horizontally homogeneous, even in the lowest few centimetres, where the fetch of wind over ' uniform ' and uniformly insolated surface is several hundred times the height above the surface. It has been suggested in Section 3(b) that this inhomogeneity might lead to a local advective flux, but the existence of the long-period fluctuations does not necessarily imply such a flux, since the temperature

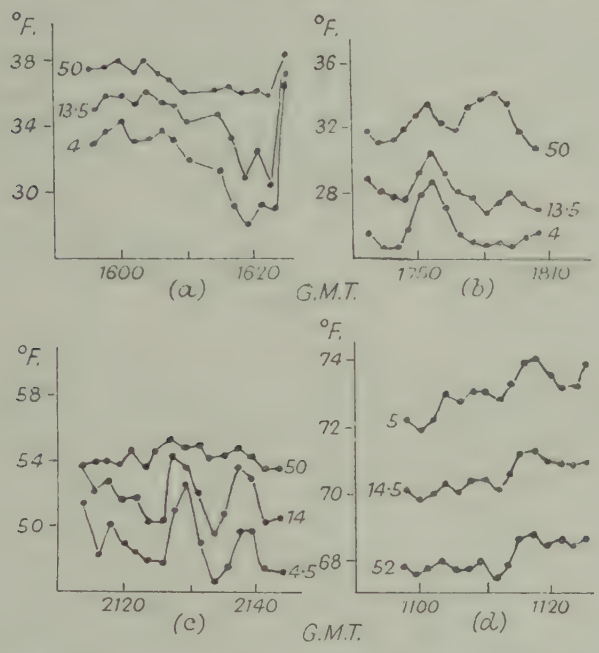


Figure 5. Examples of the fluctuations of temperature. (a) 26 Nov. 48. (b) 26 Nov. 48. (c) 10 Sept. 49. (d) 21 June 49.

not necessarily correlated with the wind fluctuations. The matter could be ettled only by simultaneous observations of temperature and horizontal and vertical ind components. For the present we can only note the existence of the fluctuations, nd the fact that they may possibly be inconsistent with the fundamental assumption f the heat-balance analysis, that the flux is proportional to the corresponding ertical gradient and to no other function of position.

It is interesting to note that the fluctuations are of about the same magnitude s those observed in similar conditions with thermometers of lag constant less than sec, as was postulated by Sutton, and maintained by him against criticism utton 1948).

(b) *The relative roles of convection and radiation in heat transfer*

In considering the variation of heat flux with height we found changes in the west metre of magnitude up to about 10 per cent. of $C(0)$. These variations were eglected when determining $C(0)$ by means of Bowen's ratio, being of the same der as errors in the gradient determinations, but they are obviously of considerable nportance when the heating and cooling of the lowest layer of air is considered. s indicated by Table II Cols. 13 and 14, the temperature changes observed are of der 1/10 to 1/100 of those which would be expected from radiative exchange one, so long as the wind is not very light nor the temperature difference tween the surface and say 100 cm very small. This implies that during the day-ne, when the lowest layer of air is being warmed by radiative exchange with the

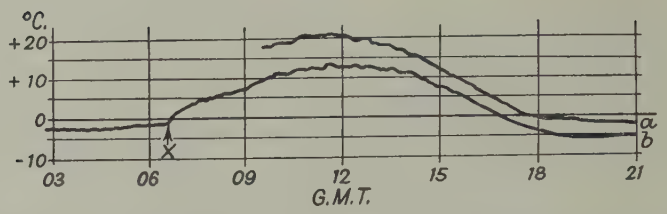


Figure 6. Records of the temperature difference between the surface and a 4 ft Stevenson screen in cloudless conditions. (a) 8 July 49. (b) 15 Aug. 49. (The irregularity at X is caused by a shadow at the site).

hot surface, convection is cooling this layer, whereas at night, when radiative exchange is cooling the air, the effect of convection is to warm the lowest layer, the resulting temperature changes being the small difference of two large opposing components. For a given humidity, so long as the wind is not very light, the change in radiative flux in the first metre is roughly proportional to $T(0) - T(100)$. Fig. 6 shows examples of this temperature difference in cloudless conditions, obtained by means of a fine thermocouple in the bulb of a glycol-filled grass thermometer,* and it will be seen that a considerable variation in radiative flux in the lowest metre throughout the 24 hr is implied. A further example has been given by Robinson (1950b). It therefore appears that in the majority of cases investigated direct radiation from the lowest layers of air is one of the controlling factors in the diurnal variation of temperature, a fact which is perhaps not generally appreciated. Table III gives a more detailed analysis of the change in radiative and convective flux in the first metre for occasions which are probably representative of mid-afternoon and early night in clear June weather with moderate winds.

TABLE III. CONVECTIVE AND RADIATIVE FLUX CHANGES, EXAMPLES WITH MODERATE WINDS

1	2	3	4	5	6	7	8
Date	Time	Z	$F(z) - F(0)$	$E(z) - E(0)$	$R(z) - R(0)$	$C(z) - C(0)$	$K(14)$
7 June 49	2125	14	$+ 1{\cdot}1 \times 10^{-6}$	$- 5 \times 10^{-10}$	$+ 6{\cdot}7 \times 10^{-6}$	$- 5{\cdot}6 \times 10^{-6}$	150
		26	$+ 2{\cdot}2 \times 10^{-6}$	$- 9 \times 10^{-10}$	$+ 1{\cdot}1 \times 10^{-5}$	$- 9 \times 10^{-6}$	
		51	$+ 4{\cdot}5 \times 10^{-6}$	$- 1{\cdot}8 \times 10^{-9}$	$+ 2{\cdot}0 \times 10^{-5}$	$- 1{\cdot}5 \times 10^{-5}$	
		101	$+ 9{\cdot}6 \times 10^{-6}$	$- 3{\cdot}6 \times 10^{-9}$	$+ 3{\cdot}5 \times 10^{-5}$	$- 2{\cdot}5 \times 10^{-5}$	
21 June 49	1400	14	$- 9 \times 10^{-7}$	$+ 3{\cdot}6 \times 10^{-9}$	$- 6{\cdot}9 \times 10^{-5}$	$+ 6{\cdot}8 \times 10^{-5}$	200
		27	$- 2{\cdot}1 \times 10^{-6}$	$+ 5{\cdot}5 \times 10^{-9}$	$- 1{\cdot}1 \times 10^{-4}$	$+ 1{\cdot}2 \times 10^{-4}$	
		52	$- 4{\cdot}9 \times 10^{-6}$	$+ 9{\cdot}7 \times 10^{-9}$	$- 1{\cdot}7 \times 10^{-4}$	$+ 1{\cdot}7 \times 10^{-4}$	
		102	$- 1{\cdot}1 \times 10^{-5}$	$+ 1{\cdot}6 \times 10^{-8}$	$- 2{\cdot}8 \times 10^{-4}$	$+ 2{\cdot}7 \times 10^{-4}$	

With very light winds different conditions obtain, as may be seen from Table IV which gives details of the change of heat flux with height for a number of occasions when the mean wind at 1 m was less than 50 cm sec^{-1}. It is seen that, although the temperature gradients were considerable, the radiative flux changed comparatively little with height, and that in the lowest levels there was an approach to radiative

* This instrument was standardized by means of a glycol-filled thermometer on the grass, which was in turn checked from time to time by the Linke-Feussner radiometer (Robinson 1950a).

ABLE IV. Convective and radiative flux changes, examples with light winds and $Ri > 1$

1 Date	2 Time	3 Z	4 $F(z) - F(0)$	5 $E(z) - E(0)$	6 $R(z) - R(0)$	7 $C(z) - C(0)$	8 $K(14)$
Nov.	1610	13·5	$+ 4{\cdot}5 \times 10^{-6}$	$+ 5{\cdot}4 \times 10^{-5}$	$- 1 \times 10^{-6}$	$+ 5{\cdot}5 \times 10^{-6}$	27
		26	$+ 8{\cdot}0 \times 10^{-6}$	$+ 1{\cdot}1 \times 10^{-5}$	$+ 1 \times 10^{-6}$	$+ 7{\cdot}0 \times 10^{-6}$	
		51	$+ 1{\cdot}2 \times 10^{-5}$	$+ 1{\cdot}9 \times 10^{-5}$	$+ 4 \times 10^{-6}$	$+ 8 \times 10^{-6}$	
		101	$+ 1{\cdot}5 \times 10^{-5}$	$+ 3{\cdot}0 \times 10^{-5}$	$+ 1{\cdot}5 \times 10^{-5}$	0·0	
Nov. 48	1745	13·5	$+ 3 \times 10^{-6}$	$+ 4 \times 10^{-5}$	$- 4 \times 10^{-6}$	$+ 7 \times 10^{-6}$	23
		26	$+ 8 \times 10^{-6}$	$+ 1{\cdot}2 \times 10^{-5}$	$- 3 \times 10^{-6}$	$+ 1{\cdot}1 \times 10^{-5}$	
		51	$+ 1{\cdot}2 \times 10^{-5}$	$+ 2{\cdot}3 \times 10^{-5}$	0·0	$+ 1{\cdot}2 \times 10^{-5}$	
		101	$+ 1{\cdot}2 \times 10^{-5}$	$+ 2{\cdot}0 \times 10^{-5}$	$+ 7 \times 10^{-6}$	$+ 5 \times 10^{-6}$	
June 49	2133	14	$+ 2{\cdot}8 \times 10^{-6}$	$+ 3{\cdot}1 \times 10^{-5}$	$- 2{\cdot}5 \times 10^{-6}$	$+ 5{\cdot}3 \times 10^{-6}$	7
		26	$+ 3{\cdot}2 \times 10^{-5}$	$+ 4{\cdot}3 \times 10^{-5}$	0	$+ 3{\cdot}2 \times 10^{-6}$	
		52	$+ 3{\cdot}0 \times 10^{-5}$	$+ 5{\cdot}8 \times 10^{-5}$	$+ 8 \times 10^{-6}$	$- 5 \times 10^{-6}$	
		102	$+ 2{\cdot}4 \times 10^{-5}$	$+ 1{\cdot}0 \times 10^{-5}$	$+ 2{\cdot}6 \times 10^{-5}$	$- 2{\cdot}4 \times 10^{-5}$	
Sept. 49	2128	14	$+ 3{\cdot}4 \times 10^{-6}$	$+ 2{\cdot}8 \times 10^{-5}$	0·0	$+ 3{\cdot}4 \times 10^{-6}$	8
		25	$+ 5{\cdot}7 \times 10^{-6}$	$+ 3{\cdot}9 \times 10^{-5}$	$+ 7 \times 10^{-6}$	$- 1{\cdot}3 \times 10^{-6}$	
		50	$+ 8{\cdot}0 \times 10^{-6}$	$+ 9 \times 10^{-5}$	$+ 1{\cdot}6 \times 10^{-5}$	$- 8 \times 10^{-6}$	
		101	$+ 9{\cdot}1 \times 10^{-6}$	$- 2 \times 10^{-5}$	$+ 3{\cdot}5 \times 10^{-5}$	$- 2{\cdot}6 \times 10^{-5}$	

quilibrium. This condition is probably typical of very calm nights. On such ights the ' long period ' fluctuations of temperature become very marked ; Fig. 5 lows a good example of this for 26 Nov. 1948, 1610 hr. These fluctuations imply nsiderable instantaneous divergence from the mean profile ; the ' radiative quilibrium ' is a mean condition, with the air at a given height being alternately armed and cooled by radiation, the mean temperature change from this cause eing zero over periods of the order 10 min or more. This control of the mean rofile by radiative transfer suggests that vertical convective transfer must be very nall, with K approaching the molecular value. Table II shows however that (0) remained comparatively high on the occasions concerned, and $K(14)$ has been bulated in Table IV. For both the November observations there is some doubt nce the surface temperature was below freezing point, and though the freezing f water on the grass was not observed it would, if present, have constituted a heat ource and led to spuriously high values of $C(0)$. This objection cannot be made the other two cases, where K, though very small, is still some 50 times the mole- ular value.

The co-existence of appreciable convective flux at the surface and radiative ntrol of the temperature profile suggests that the convective flux is almost wholly dvective, and it seems likely that the process which we have described as local r small-scale advection dominated the heat exchange on these occasions ; this view ccords with the nature of the temperature fluctuations, and implies breakdown f the assumptions made in defining K.

(c) *Humidity changes – the formation of ground fog*

It can be seen from Table II Cols. 12 and 16 that the change with height of he water-vapour flux is normally very small compared with $E(0)$. Its meteorological mportance becomes apparent when it is realised that a value of -1×10^{-8} g cm^{-2} sec^{-1} or $E(100) - E(0)$ implies an increase of water content of 0·36 g m^{-3} hr^{-1} and that ·1 g m^{-3} of condensed water can form a thick fog.

Any investigation of fog formation is concerned with the change in water-vapour flux with height. Emmons and Montgomery (1947) in a preliminary note have considered fog formation near the surface, regarding it as a problem in eddy diffusion, equating eddy conductivity and diffusivity, and considering the effects of molecular conduction and diffusion in a saturated layer very close to the surface. They conclude that loss of water vapour to the surface would be greater than loss of heat, and that fog should not form by diffusion alone. They suggest that fog formation in these conditions involves some loss of heat by direct radiation from the air. We have seen, in Section 6(b), that on the nights most suitable to fog formation the air close to the ground was neither gaining nor losing heat by radiative exchange. In two of these cases, 26 Nov. 1949, 1745 hr and 10 Sept. 1949, 2128 hr, fog did in fact form during the course of the observation. In the former case a thick fog with an extremely well-marked horizontal top began to form close to the ground at 1750 hr, and by 1805 hr, its top was above 2 m. The profile of Fig. 2 applies to the 10 min period immediately before fog formation was first observed. On 10 September 1949, the fog was much thinner and patchy, and did not persist. It was first observed at 2125 hr, about the middle of the period to which the profile of Fig. 2 refers, and drifted slowly in patches during the remainder of the observation. There seems little doubt that in these two cases fog formed in air which was not losing heat by direct radiation, and in which we have suggested that the main process of heat and water vapour exchange was by horizontal rather than vertical mixing. These considerations suggest that it may be misleading to regard ' radiation fog ' formation as a problem of turbulent exchange in a horizontally homogeneous body of air with the fluxes assumed proportional to the corresponding vertical gradients.

The time variations of temperature on these two occasions of fog formation have some points of interest, and are shown in Fig. 5. A common feature is the rise of temperature at the time when fog formation was first noted. This seems too great to be caused by the release of latent heat (condensation of 0·5 g m^{-3} could cause a temperature rise of about 1°C at the most), and may merely indicate that fog forms on the advection of a supply of warmer saturated air, in accordance with the speculations which we have advanced. The observations are insufficient for critical discussion, but are reported now since further similar material cannot be expected to accumulate rapidly.

7. The variation of the gradients with height

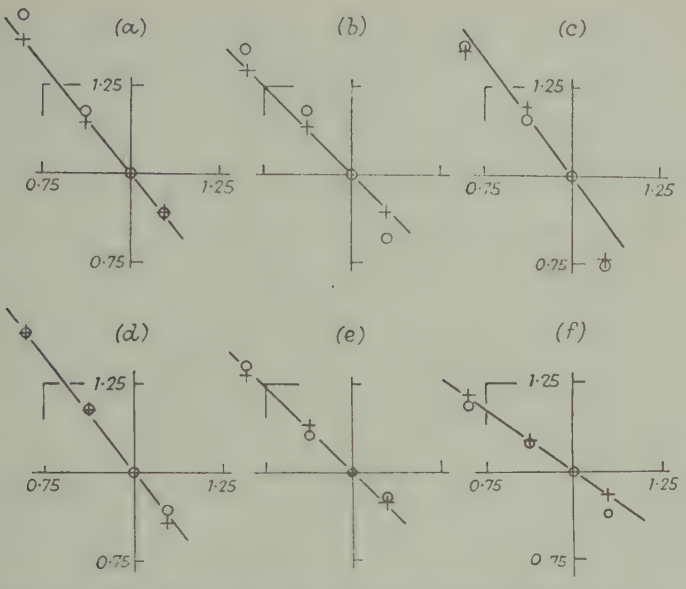
Deacon (1948,1949) found that to a high degree of precision the wind gradient over a short grass surface between heights of 0·5 m and 8 m could be represented by a power law of the type.

$$\partial u/\partial z = az^{-\beta}$$

He further demonstrated a general relation between β and the stability which implied

$$\left.\begin{array}{l}\beta > 1 \text{ for } Ri < 0\\ \beta < 1 \text{ for } Ri > 0\end{array}\right\} \quad . \quad . \quad . \quad (11)$$

and was consistent with Eq. (8) for $Ri = 0$. Sheppard (1947) and Pasquill (1949c), working in the height range from 25 cm to 2 m, confirmed that u varied with z



igure 7. Illustrating the variation of the gradients of temperature and wind with height in stable d unstable conditions. Abscissae $-\log_{10}(z/20)$; ordinates $-\log_{10}\{(\partial u/\partial z)/(\partial u/\partial z)_{20}\}$ (crosses) d $\log_{10}\{\partial T/\partial z)/(\partial T/\partial z)_{20}\}$ (circles). Stable conditions. (a) 5 Sept. 49, 2201 GMT. (b) 12 Sept. 49, 00 GMT. (c) 28 Sept. 49, 1905 GMT. Unstable conditions. (d) 21 June 49, 1111 GMT. (e) 21 June 49, 1400 GMT. (f) 23 June 49, 1636 GMT.

nore rapidly than log z for $Ri > 0$, and less rapidly than log z for $Ri > 0$. The resent measurements, which were made closer to the surface, were examined to e if the same relation was obeyed. Only those occasions for which the gradients ad been obtained by numerical differentiation were used, and in Fig. 7 plots are own of log z against log $\partial T/\partial z$ and log $\partial u/\partial z$ for a number of these cases. It will e seen that the law established by Deacon at greater heights does not hold for the resent observations at small heights, for although the power law for $\partial u/\partial z$ is dicated sufficiently clearly, the relations in Eq. (11) are not obeyed, in fact we have $\geqslant 1$ for each instance of $Ri > 0$, whilst for $Ri < 0$ there are a number of examples ith $\beta \leqslant 1$. The two cases discussed in Section 6, which gave anomalous results r the fluxes by the aerodynamic method, have $Ri < 0$ and $\beta \leqslant 1$, but on 23 June 949, 1636 hr, with $Ri < 0$, $\beta < 1$ the aerodynamic and heat-balance flux computaons were in agreement, so that failure of Deacon's law is not necessarily accomanied by failure of the aerodynamic method. It is not certain in any of the examples hown in Fig. 7 that the indicated differences in behaviour of temperature and wind radient are significant in view of the uncertainties of observation.

8. The critical value of Richardson's number

The wind profiles have some interesting features on the occasions of very light ind when there was radiative equilibrium in the lowest layers. The profiles for nese four cases are plotted in Fig. 8, and show an approach to a linear relation

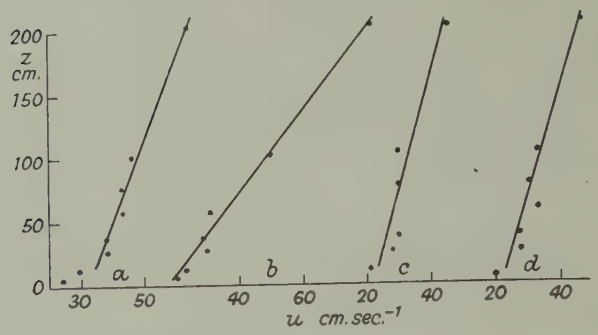


Figure 8. Wind profiles in very light winds. (a) 26 Nov. 48, 1610 GMT. (b) 26 Nov. 48, 1745 GMT. (c) 17 June 49, 2133 GMT. (d) 10 Sept. 49, 2128 GMT.

between u and z. The arguments of Section 2(f) must be kept in mind when interpreting these profiles ; there was almost certainly some stopping of the anemometer cups at all heights on each occasion. The approach to a linear profile is however interesting in view of the suggested absence of vertical mixing, as is the fact that $Ri > 1$ on these and no other occasions. Ri can only be estimated very approximately because of the scatter of the wind observations, but suggested values for $Ri(25)$ are :

26 Nov. 1948	1610 hr	$Ri\,(25) = +1{\cdot}4$
26 Nov. 1948	1745 hr	$Ri\,(25) = +2{\cdot}1$
17 June 1949	2133 hr	$Ri\,(25) = +2{\cdot}5$
10 Sept. 1949	2128 hr	$Ri\,(25) = +6{\cdot}5$

In view of the uncertainty concerning the critical value of Ri in atmospheric flow (see *e.g.*, Sutton 1949, p. 91) it is interesting to examine some other occasions of positive Ri. On 10 Nov. 1948, 1750 hr with $Ri\,(25) = +0{\cdot}4$ and $u < 50$ cm sec^{-1} for $z < 50$ cm, the wind profile, based on four observations only, was certainly non-linear, and the convective and radiative flux changes, though small up to 25 cm, each accounted for about half the observed temperature change. On 24 Nov. 1948, 1755 hr Ri (25) was $+0{\cdot}25$, the wind profile was normal, being satisfied quite

TABLE V. CONVECTIVE AND RADIATIVE FLUX CHANGES, EXAMPLES WITH LIGHT WINDS AND $1 > Ri > 0$

1	2	3	4	5	6	7	8
Date	Time	Z	$F(z) - F(0)$	$E(z) - E(0)$	$R(z) - R(0)$	$C(z) - C(0)$	$K(14)$
10 Nov. 48	1750	13·5	$+3{\cdot}6 \times 10^{-6}$	$+4 \times 10^{-9}$	$+1{\cdot}7 \times 10^{-6}$	$+2 \times 10^{-6}$	20
		26	$+8{\cdot}1 \times 10^{-6}$	$+9 \times 10^{-9}$	$+4{\cdot}1 \times 10^{-6}$	$+4 \times 10^{-6}$	
		51	$+1{\cdot}6 \times 10^{-5}$	$+1{\cdot}7 \times 10^{-8}$	$+1{\cdot}1 \times 10^{-5}$	$+5 \times 10^{-6}$	
		101	—	—	—	—	
24 Nov. 48	1755	13·5	$+6 \times 10^{-7}$	—	$+2{\cdot}4 \times 10^{-6}$	-2×10^{-6}	60
		26	$+1{\cdot}0 \times 10^{-6}$	—	$+1{\cdot}0 \times 10^{-5}$	-9×10^{-6}	
		51	$+1{\cdot}5 \times 10^{-6}$	—	$+1{\cdot}6 \times 10^{-5}$	$-1{\cdot}5 \times 10^{-5}$	
		101	$+3{\cdot}0 \times 10^{-6}$	—	$+3{\cdot}7 \times 10^{-5}$	$-3{\cdot}4 \times 10^{-5}$	

losely by $\partial u/\partial z \propto z^{-0\cdot8}$ and the radiative-flux change up to 25 cm was ten times reater than the total flux change indicated by the temperatures. Details of the ux changes for these two occasions are given in Table V. The indications of the resent observations are therefore that for $Ri = +0\cdot25$, vertical turbulence is still ctive, for $Ri = +0\cdot4$ it is probably present but not marked, whilst for $Ri > +1\cdot4$ is negligible.

9. Conclusions

The main argument which we have advanced is that in most cases the classical ethod of Bowen, when applied to observations taken over short grass, yields the ame results for the convective fluxes of sensible and latent heat as does an aero- ynamic method of computation, essentially similar to that favoured by Pasquill. oth these methods involve the conception of the eddy-diffusion coefficients, and e have pointed out the lack of direct support for this, and the difficulties introduced y the undoubtedly important advective term. Bowen's method assumes equality f eddy conductivity and eddy diffusivity ; the aerodynamic method, equality of ddy diffusivity and eddy viscosity. Agreement of the two methods indicates quality of all three coefficients, in accordance with the assumptions of the simple mixing-length theory in connection with which they were first introduced. This quality could be established directly by simultaneous observation of the three ppropriate fluxes and gradients, which we have not attempted. We have however stablished a strong probability that the ratio of eddy diffusivity to eddy conductivity emains constant with height within the layer investigated, from which it is a small tep to the assumption of equality as in Bowen's method.

The question of change of heat flux with height has been examined using the adiation chart, and we have shown that the change of temperature of the lowest ayers of air is normally the small resultant of much larger tendencies due to changes n the radiative and convective fluxes acting in opposite directions. It does however ppear that in very light winds (such as are only experienced at night) the mean hermal state of the layer of air very near the surface is one of radiative equilibrium. Coupled with the large values of Richardson's number on these occasions this has been taken to indicate the absence of vertical convection in the layer concerned, in spite of comparatively large values of the convective flux, which we have suggested are connected with horizontal mixing. Such conditions are normally associated with the occurrence of radiation fog and frost, and we conclude that arguments employing the eddy coefficient conception may be misleading when applied to these problems. If this interpretation of the results is conceded the critical value of Richardson's number for the suppression of vertical turbulence appears to be between 0·5 and 1·5.

Acknowledgments

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	1949	*Atmospheric Turbulence*, London (Methuen).

Appendix I

Mean values of temperature, humidity and wind on the occasions investigated

Date and time	z cm	T °F	χ g m^{-3}	u cm sec^{-1}	Date and time	z cm	T °F	χ g m^{-3}	u cm sec^{-1}
19 July 48	0	102			10 Nov. 48	0	36·5		
1400	5	77·77	13·09		1735	4	39·04	6·18	
1430	8	77·37	12·40		1805	5			26
	14	76·61	12·18			7	39·97	6·24	
	25	75·96	12·06	287		12			33·5
	50	75·32	11·74	323		13·5	40·89	6·41	
	100	74·84		349		26	41·31	6·47	
	200	74·56	11·33	385		27·5			40
						45			46
2 Oct. 48	0	83				51	41·84	6·53	
1055	4·5	65·07	11·66			101	42·48	6·64	
1125	7·5	64·29	11·13			201	43·33	6·85	
	13·5	63·31	10·96						
	26	62·72	10·75						
	51	62·12	10·33						
	101	61·79	10·48						
	201	61·51	10·03						
					11 Nov. 48	0	49·5		
10 Nov. 48	0	47			1400	4	51·34	8·48	
1455	4	47·22	7·28		1430	5			72
1525	5			78		7	51·57	8·25	
	7·5	47·60	7·23			12			88·5
	12			101		13·5	51·65	8·24	
	13·5	47·92	7·29			26	51·78	8·17	
	25			116		26·5			104
	26	48·17	7·26			37·5			110·5
	30			130		51	51·82	8·07	
	50·5	48·45	7·24			80			123·5
	101	48·65				101	51·89	8·16	126
	201	48·83	7·23			201	51·90	8·01	141·5

APPENDIX I (*continued*)

Mean values of temperature, humidity and wind on the occasions investigated

ate and ime	z cm	T °F	χ g m^{-3}	u cm sec^{-1}	Date and time	z cm	T °F	χ g m^{-3}	u cm sec^{-1}
Nov. 48	0	30			7 June 49	0	50		
1740	4	34·47	5·00		2110	4·5	55·13	9·29	
1810	5			32·0	2140	5·5			119
	7	35·71	5·14			7·5	55·40	9·26	
	12·5			48·2		11·5			217
	13·5	36·80	5·22			14	55·88	9·29	
	26	37·76	5·31			26	56·18	9·29	
	51	38·61	5·37			26·5			257
	57·5			74·6		38			272
	101	39·29	5·49	89·6		51	56·43	9·34	
	201	40·22	5·54	107·2		57·5			286
						75			296
						101	56·63		301
						201	56·74	9·34	
Nov. 48	0	29·5							
1555	4	32·05	4·78						
1625	5			24					
	7	33·25	4·89		15 June 49	0	44		
	12·5			29	2115	5	49·98	7·80	54
	13·5	34·38	5·14		2145	8	50·64	7·65	
	26	35·55	5·37			12			77·5
	26·5			38		14	51·26	7·68	
	37			38		26·5			103
	51	36·86	5·57			27	51·74	7·61	
	57·5			43		37			110
	76			43		52	52·23	7·55	
	101	37·89	5·83	46		57			120
	201	39·92	6·09	64		76·5			125
						102	52·59	7·55	131
						202	52·90	7·50	146·5
Nov. 48	0	24·5							
1740	4	25·16	3·74						
1750	5			20	16 June 49	0	69		
	7	26·52	3·94		1605	5	61·84	8·23	79·5
(1740	12·5			23	1635	8	61·58	7·86	
-1810	13·5	28·15	4·20			12			105
for u)	26	29·61	4·42	34		14	61·33	7·71	
	37			32		27	60·99	7·49	131
	51	31·35	4·68			37			136
	57·5			37		52	60·81	7·35	
	101	32·85	4·96	50		76·5			158·5
	201	34·60	5·26	82		102	60·59	7·31	154
						202	60·45	7·18	171·5
3 May 49	0	76			17 June 49	0	42		
1352	5	63·31	10·76	240	2118	5	45·93	7·77	
1422	8	63·06	10·47		2148	8	46·66	7·55	
	12			310		12			21
	14	62·69	10·38			14	48·44	7·78	
	27	62·10	10·18	429		25			28
	38			453		26	49·56	7·80	
	52	61·34	9·77			37			30
	57·5			486		52	50·94	7·89	
	75			500		76			30
	102	61·23	9·73	515		102	52·23	7·90	30
	202	60·84	9·57	577		202	53·88	7·90	46

APPENDIX I (*continued*)

Mean values of temperature, humidity and wind on the occasions investigated

Date and time	z cm	T °F	χ g m^{-3}	u cm sec^{-1}
18 June 49	0	82·5		
1106	4·5	66·56	9·70	
1136	5			136
	7·5	65·43	8·41	
	12			183
	14	64·28	8·53	
	25			235
	26·5	62·91	8·56	
	37			238
	52	61·95	8·18	
	57			255
	76			260
	102	61·02	8·07	276
	202	60·63	7·89	295
20 June 49	0	102		
1106	5	69·04	10·49	108
1136	8	67·45	9·70	
	12			137
	14	65·87	9·29	
	25			164
	27	64·65	9·05	
	37			168
	52	63·68	8·65	
	57·5			180
	76			183
	102	62·97	8·67	191
	202	62·59	8·42	204
20 June 49	0	97·5		
1355	5	74·38	10·25	106
1425	8	73·40	9·65	
	12			140·5
	15	72·67	9·44	
	25			170·5
	27	71·53	9·05	
	37			179
	52·5	70·67	8·64	
	58			183·5
	76			192
	102	69·99	8·43	203
	202	69·59	8·23	215
21 June 49	0	101		
1056	5	73·09	11·08	135·5
1126	8	71·71	10·42	
	12			178
	14·5	70·52	10·13	
	25			213
	27	69·13	9·75	
	37			228
	52	68·06	9·41	
	58			244
	76			250
	102	67·19	9·16	263
	202	66·70	9·05	281
21 June 49	0	106		
1345	5	77·81	11·57	126
1415	8	77·15	10·95	
	12			165
	14	75·87	10·61	
	25			194
	27	74·45	10·33	
	37			215
	52	73·39	10·05	
	58			226
	76			229
	102	72·64	10·00	239
	202	72·35	9·61	259
21 June 49	0	87		
1556	5	76·31	11·55	
1626	8	75·88	11·16	
	12			191
	145	75·21	10·96	
	25			248
	27	74·57	10·87	
	37			264
	52	74·00	10·57	
	58			274
	76			286
	102	73·54	10·58	299
	202	73·20	10·30	319
23 June 49	0	103		
1048	5	73·01	9·73	184
1118	8·5	71·96	9·34	
	12			240
	14·5	70·72	9·18	
	25·5			308
	27·5	69·34	8·98	
	37			327
	52·5	68·25	8·71	
	58·5			343
	75·5			354
	102	67·45	8·65	373
	202	66·83	8·44	397
23 June 49	0	93		
1417	5	73·29	9·08	211
1447	8·5	72·47	8·77	
	12			282
	14·5	71·58	8·67	
	25			348
	27.5	70·58	8·48	
	37			368
	52·5	69·74	8·21	
	58			397
	76			408
	102	69·11	8·23	422
	202	68·79	7·92	454

APPENDIX I (*continued*)

Mean values of temperature, humidity and wind on the occasions investigated

Date and time	z cm	T °F	χ g m^{-3}	u cm sec^{-1}
23 June 49	0	84		
1621	4·5	70·04	9·27	
1651	5			187
	7·5	69·52	9·01	•
	12			248
	14	69·01	8·93	
	25			320
	27	68·34	8·77	
	37			327
	52	67·90	8·63	
	58			352
	76			351
	102	67·56	8·96	377
	202	67·47	8·42	405
24 June 49	0	107·5		
1404	4·5	75·68	7·65	
1434	5			120
	7·5	74·04	7·00	
	12			158·5
	14	72·67	6·73	
	25·5			186
	27	71·33	6·22	
	37			197·5
	52	70·42	5·87	
	57·5			204
	76·5			201
	102	69·77	5·62	216
	202	69·35	5·44	229
1 July 49	0	110		
1107	5	77·61	10·04	129
1137	8	76·66		
	12			168
	14·5	75·62	9·45	
	25·5			207
	27·5	74·34	9·10	
	37			194
	52·5	73·22	8·68	
	57·5			223
	76·5			220
	102	72·52	8·54	241
	202	72·09	8·47	262
1 July 49	0	104		
1413	5	80·71	9·61	121
1443	8	79·99	9·10	
	12			156
	14·5	79·11	8·93	
	25·5			189
	27·5	77·90	8·70	
	37			194·5
	52·5	77·08	8·28	
	57·5			209
	76·5			212
	102	76·39	8·14	223
	202	75·97	7·88	242
5 Sept. 49	0	58·5		
2146	4·5	64·25	12·57	
2216	5			86
	7	64·95	12·39	
	12			113·5
	14	65·66	12·50	
	23			128
	25	66·07	12·50	
	37·5			137
	50·5	66·45	12·48	
	53			148
	101	66·68	12·52	152·5
	201	66·67	12·54	167·5
10 Sept. 49	0	45·5		
2113	4·5	48·91	9·07	
2143	5			20
	7	49·83	8·99	
	14	51·80	9·92	
	25	53·06	10·22	27·5
	37			27·5
	50·5	54·19	10·60	
	56			33·5
	76			30·5
	101	55·15	10·89	33·5
	201	56·69	11·50	47
12 Sept. 49	0	59		
2145	4·5	62·18	12·77	
2215	5			179
	7	62·47	12·59	
	12			232
	14	62·74	12·67	
	25	63·04	12·71	
	37·5			302
	50·5	63·22	12·69	
	53			309
	76			325
	101	63·39	12·76	338
	201	63·51	12·82	373
28 Sept. 49	0	57		
1850	5	61·25	11·78	137
1920	8·5	61·77	11·73	
	12			193
	14·5	62·21	11·79	
	23			230
	27	62·44	11·82	
	37·5			247·5
	52	62·71	11·88	258
	71			265·5
	101	62·78	11·88	276

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The production of showers by the coalescence of cloud droplets

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SUMMARY

The growth of unusually large cloud droplets by coalescence within isolated convective clouds is examined. It is found that if their initial radii are about 20 μ or more then growth into raindrops may readily occur. Raindrop nuclei of this size may be provided by spray in maritime air masses, but there is reason to suppose that in continental air masses droplets of the required initial size are not naturally introduced into the bases of convective clouds. Therefore the coalescence mechanism of precipitation release is probably effective only in maritime air masses. A condition for the production of a shower by the coalescence mechanism is obtained and applied to predict a minimum critical cloud depth of about 1½ to 2 km. Showers may develop from clouds wholly below the 0°C isotherm if their base temperature exceeds about 8°C. A relation between the size of raindrops and the speed of the convective updraught is found, and an association is discovered between the non-fulfilment of conditions for raindrop-splitting within cloud and the absence of thunder.

1. INTRODUCTION

In all droplet-clouds there is a tendency for the droplets to coalesce. Even if at first they are all the same size, then providing that they do not all carry like electrical charges some collisions will eventually be caused by Brownian motion, by the hydrodynamic attraction between droplets which chance to be falling close together, or by small-scale turbulence in the air. A potentially much more effective mechanism, overtaking due to differences in fall-speeds, is then introduced and operates with increasing rapidity as larger and larger droplets are formed.

Findeisen (1939) was the first to consider quantitatively this process as a mechanism of rain production. He believed, however, that it has not the efficiency in nature which his theory indicated, and thought that his calculations were invalidated because a large drop does not catch all of the smaller droplets in its path (as he had assumed), but brushes many aside.

Langmuir (1948) greatly advanced the theory, showing how to allow for this effect by calculating the ' efficiency of catch ' of the drop, and discussed quantitatively the development of chain reactions of raindrop multiplication following the artificial introduction of large water drops into the upper parts of convective clouds. He did not suggest that shower rain could arise naturally by this mechanism alone, but thought that melting hail might be an important agent in introducing large drops into the lower parts of storm clouds.

Houghton (1950) has used Langmuir's formulae for efficiency of catch to compare the rate of growth of drops by coalescence with the rate of growth of ice crystals by condensation.

Bowen (1950) has calculated numerically the trajectories in convection clouds of drops growing by coalescence under particular conditions (the growth in this way is assumed to commence following the chance combination of some pairs of droplets in the lower parts of the clouds). He demonstrates that the process may reasonably result in the production of raindrops in clouds several thousand feet

deep, and describes observations which show that in Australia showers frequently develop in the way his theory suggests.

The aim of the present paper is the derivation of generalized relations concerning the initiation of showers by the coalescence mechanism, and the size of the raindrops it produces, throughout the range of meteorological conditions associated with convective clouds. We begin with a statement of the important large-scale characteristics of these clouds.

2. The characteristics of isolated cumuliform clouds

Cumulus clouds are usually short-lived ; often they appear, reach upwards a kilometre or so, decay and evaporate, all within twenty minutes. Larger and deeper clouds may persist as recognizable entities for an hour or more, but are assemblages of individual cloud masses which have a much briefer existence, continually appearing below the main bulk and quickly rising into prominence, only to sink aside and evaporate. It appears that the life of the small particles in such clouds is likewise limited, rarely exceeding about half an hour unless they are transformed into the ice particles of anvil clouds, whose decay may extend over many hours.

Two other features of importance are the updraught and the liquid-water concentration within the clouds. The speed of the updraught within small clouds (depth about a kilometre) is observed to be about one to three m/sec ; in large clouds it is often greater and occasionally exceeds 10 m/sec in their upper parts. Inside such clouds, which may be a kilometre or more across, the condensation thus proceeds so rapidly that it may be considered an adiabatic process, and the liquid-water concentration at any level can be estimated by assuming that the cloud air rises from the base, cooling at the saturated adiabatic rate with respect to liquid water. The concentration of ice particles appearing in the cloud is extremely small until the temperature has fallen to about $-30°C$, so that the estimate is valid even at levels considerably above that of the $0°C$ isotherm, and gives values in the interior of the clouds which vary from about 1 g/m^3 in cold weather to about 6 g/m^3 in tropical weather. Near the edges of large clouds, and perhaps throughout very small clouds, mixing with the unsaturated air of the environment may considerably reduce the liquid-water concentration.

The abundance and properties of atmospheric nuclei are such that in surface air rising into cloud at speeds of 1 m/sec or more several hundred droplets arise in each cm^3. As their number does not change once the air has risen some hundred or so metres above the cloud base, and as there is some tendency for them to become of nearly uniform size while *condensation* continues (Howell 1949), their average size at any level can be determined from their initial concentration and the liquid-water content. Their size near the cloud base may then be under-estimated if they are hygroscopic and already have radii of a few microns at a relative humidity of 100 per cent. At higher levels the error is negligible, and reasonable values of nucleus concentration lead to average droplet radii well within the cumulus which vary between about 7 and 20 μ.

It has already been shown that in a population of droplets of this size the only factor leading to collisions which need be considered is the difference in the fall-speeds of droplets of somewhat different sizes (*e.g.*, Schumann 1940). As a small

increase in this difference may greatly accelerate the process, and as a large raindrop is composed of some 10^7 cloud droplets, the growth of only the largest among such a number may be of concern, *i.e.*, the largest in a volume of about fifty litres. If a chain reaction of raindrop multiplication can occur within the cloud then the presence of unusually large droplets in concentrations of $1/m^3$, or even $1/1000$ m^3, may be important.

3. The growth of large hygroscopic nuclei

Unusually large droplets may arise in the presence of especially favourable condensation nuclei, such as are known to exist in maritime air masses. Here hygroscopic nuclei containing about 10^{-13} g of sea salt occur in concentrations of several hundred per cm^3 (Wright 1940 ; Dessens 1947 ; Woodcock 1950) ; amongst these are a few relatively large nuclei, containing up to rather more than 10^{-9} g of salt, in concentrations of the order 1/litre (Findeisen 1937 ; Woodcock and Gifford 1949 ; Woodcock 1950). Such large nuclei originate as spray droplets having radii of about 20 to 30 μ ; when they are introduced into air with a relative humidity of 90 per cent or less they shrink towards equilibrium radii of about 5 to 10 μ. If, however, they are lifted towards a cumulus cloud then they are subjected to an increasing humidity and some growth may occur. The growth or evaporation of nucleus-droplets of this size proceeds rather slowly, and it cannot be assumed that their radii are always close to the equilibrium radii, which in the saturated air at the cloud base may exceed 100 μ.

To illustrate this we may consider the growth of a large nucleus containing $6{\cdot}10^{-10}$ g of salt, which, having reached equilibrium with a radius of about 6 μ* in an atmosphere of about 60 per cent relative humidity, is then plunged into saturated air. The difference δp between the vapour density in the environment (p) and at the droplet surface is given by the relation (see, *e.g.*, Wright 1936)

$$\frac{\delta p}{p} = \frac{Q}{r^3} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where r is the droplet radius and Q is a constant numerically equal to about one-sixth of the mass of salt it contains (the direct effect of the curvature on the vapour pressure at the droplet surface may be neglected with droplets of the size considered). Thus $Q \simeq 10^{-10}$ and from the usual relation

$$r\frac{dr}{dt} = k\delta p \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where k, the coefficient of diffusion of water vapour, is approximately 0·25 cm^2/sec, we obtain

$$r^4\,dr = 3{\cdot}75 \times 10^{-16}\,dt \quad . \quad . \quad . \quad (3)$$

using a high value of p (15×10^{-6} g/cm^3, corresponding to a temperature of about 20°C) and ignoring the reduction in the growth rate due to the warming of the droplet by the heat of condensation. For small values of the initial radius the integration of Eq. (3) gives the approximate relation for the radius in microns after a time t :

$$r_\mu = 11\,t^{1/5} \quad . \quad . \quad . \quad . \quad (4)$$

* The equilibrium radius may be calculated in the way described by Wright (1936).

Thus after a day the radius would be about 110 μ, barely a third of the equilibrium radius.

The period available for the growth of a nucleus during ascent from the surface to the cloud base is of the order 1,000 sec, and during this time the humidity is increased only gradually towards saturation. It may therefore be anticipated that very little change of radius occurs during this ascent, however large or small the nucleus is initially.

This has been confirmed by considering the growth of a large nucleus, initially in equilibrium at 62·7 per cent R.H., when the relative humidity is increased uniformly at a rate corresponding to a speed of rise towards a cumulus base (R.H. 100 per cent) of 1 m/sec. The calculation was made by integration of the growth equation, using the Taylor series method, and the result is shown in Fig. 1. Upon reaching the cloud base the droplet radius has increased by only about 2 μ to a little over 8 μ, whereas the equilibrium radius at this level is about 305 μ.

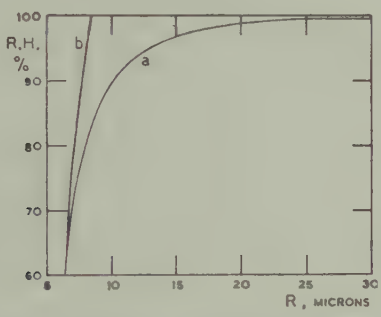


Figure 1. The radius of a droplet containing 6×10^{-10} g of sea salt as a function of relative humidity, under equilibrium conditions (curve *a*), and when lifted at 1 m/sec towards a cumulus, from equilibrium at 62·7 per cent R.H. (curve b). The radius increases from 6·452 μ to 8·375 μ.

The further growth within the lower parts of the cloud is also very small, being restricted there because of the removal of condensing vapour by the overwhelmingly more numerous small nuclei which have become activated.

Now over the ocean and near coasts, where spray is continuously formed, the air may be expected to contain significant numbers of nucleus-droplets which have so recent an origin that their radii are still some 20 or 30 μ, and during the formation of a cumulus they would enter the cloud with this size. It is reasonable to expect that such droplets are present in practically all unstable maritime air masses, because the instability is usually due to the rapid advection of cold air masses, and is therefore accompanied by winds strong enough to raise waves and break them into spray. On the other hand, as these air masses move inland the large nucleus-droplets disappear by settling-out, or they shrink slowly to approximate after some hours to equilibrium radii of about 5 to 10 μ. Thus even if a number remain, they revert to a condition in which they are inefficient agents in the production of especially large cloud droplets. The hygroscopic nuclei which are produced inland by natural and artificial combustion and industrial processes are generally believed to be very much smaller even than sea-salt nuclei, although no attempts have been made to

discover the largest present in volumes of a litre or more of air. Non-hygroscopic nuclei would seem at first to be of even less importance, but it is possible that very large dust particles could initiate the coalescence of cloud droplets without themselves being involved in condensation.

In the following paragraphs we examine the growth of rare droplets which are introduced into cumulus with radii of 20 to 40 μ ; we may be confident that the results have application in unstable maritime air masses, and will consider later their significance in other situations.

4. The growth of drops by coalescence

The growth by coalescence of a droplet of radius R as it falls at a speed V through a cloud of smaller droplets of uniform radius r and fall-speed v is described by the equations :

$$4\pi R^2 D\, dR = \pi R^2 E\,(V - v)\, m\, dt$$

or, with D, the density of water, equal to 1,

$$4dR = E\,(V - v)\, m\, dt \quad . \quad . \quad . \quad (5)$$

and

$$dz = (U - V)\, dt \quad . \quad . \quad . \quad . \quad (6)$$

where :

E is the fraction of cloud droplets in the path (*i.e.*, the projected cross-section) of the large droplet with which it collides, m is the concentration of liquid water in the form of cloud droplets, z is height above some fixed level. U is the speed of the updraught and t denotes time.

The assumption is made that every collision is followed by coalescence. It must be acknowledged that there is some doubt about this. From the experiments of Rayleigh (1920) and Findeisen (1932) it would appear that a small uniform electric charge on the droplets greatly hinders their coalescence, and probably for this reason attempts to observe it in clouds produced by atomisation have been unsuccessful. On the other hand Rayleigh found atmospheric dust to be a powerful agent in promoting coalescence, and it may be observed under a microscope that coalescence readily occurs when two small droplets held on fine slack threads are gently brought together. The results of the calculations do not suggest that the hypothesis adopted is at all unreasonable.

In the lower parts of the cloud the fall-speed of the cloud droplets is less than 1 cm/sec, whereas a droplet of radius 20 μ has a fall-speed of 4·5 cm/sec and one of radius 30 μ a fall-speed of about 11 cm/sec. Higher in the clouds we shall be concerned with larger droplets whose fall-speed is even greater when compared with that of the cloud droplets, which in a first approximation may therefore be ignored. We may then eliminate t from Eqs. (5) and (6) to obtain

$$4\frac{(U-V)}{EV}\, dR = m\, dz \quad . \quad . \quad . \quad . \quad (7)$$

V may with small error be regarded as a function of R only, and U may be considered to be a constant, implying a uniform updraught. If now E can be

garded as constant, or as a function solely of z or of R, the equation can be inte- ated throughout the history of a droplet, from the time it enters the cloud until falls out as a raindrop ; in the last instance we have

$$4\int_{R_0}^{R_1}\frac{U-V}{EV}\,dR=\int_{z_0}^{z_0}m\,dz=0 \quad . \quad . \quad . \quad (8{\cdot}1)$$

hile in the others

$$4\int_{R_0}^{R_1}\frac{U-V}{V}\,dR=\int_{z_0}^{z_0}E\,m\,dz=0 \quad . \quad . \quad . \quad (8{\cdot}2)$$

Vhen the growth of rare drops is considered, the integrals on the right of the quations vanish because m may be regarded as independent of time ; at every vel in the cloud equal and opposite contributions to the integrals are then made uring the ascent of a drop and during its later descent when it is large enough fall through the updraught towards the ground. If the concentration of growing indrops increases to the point at which they interfere with one another by appreci- bly modifying m this simple treatment is no longer valid. Consideration of this evelopment stage of shower rain, as distinct from its initiation, is left to a future aper. If justifiable the above equations show that during at least the initial stage f shower production the radius of a raindrop falling out of the cloud is a function nly of the size with which it originally entered the cloud and the speed of the pdraught.

It appears, however, that E is likely to be a function of both R and z, since depends upon the size of both the raindrop and the cloud droplets and upon heir speed of approach. Langmuir (1948) has obtained formulae for E which can e used with confidence providing r is not comparable with R. He calculates the rajectories of droplets (regarded as points) which are swept towards a spherical ollector, and defines E as $(x/R)^2$ where x is the original distance from the pro- cted collector-centre of a droplet whose trajectory just grazes the surface of the ollector (Fig. 2). The value E_L thus obtained is an under-estimate if the radius f the droplet cannot be ignored. We may consider a better estimate to be given y assuming that a droplet originally farther by its own radius from the grazing rajectory moves on a parallel path, leading to $E_1=(x+r)^2/R^2=(E_L^{1/2}+r/R)^2$, hich approximates to Langmuir's value when r/R becomes small.

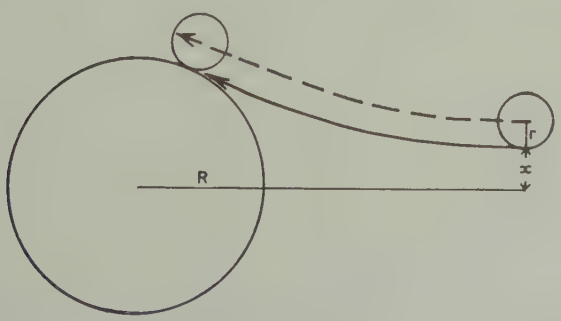


Figure 2.

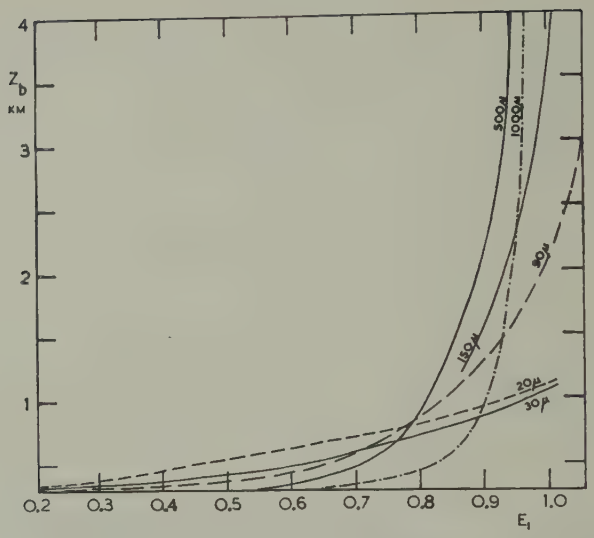


Figure 3. E_1, an estimated efficiency of catch, for droplets of several radii as a function of height Z_b above the base (temperature 20°C) of a cloud containing uniform droplets in a concentration of 5×10^4/g of air.

Values of E_1 as a function of z_b, height above the base of a typical cloud, are plotted in Fig. 3 for several values of R. E_1 increases very rapidly just above the cloud base, but is practically confined between limits of 0·75 and 1·0 when z_b exceeds about 0·6 km. This is largely due to the changes with height of r ; up to this level r increases very rapidly from zero to about 8 μ, a critical size above which E_1 varies little with R. Naturally when R is small there is considerable uncertainty about the accuracy of E_1 as a measure of E, but the exact value of E in the lowest part of the cloud happens to be of little account in the present problem. This is because in this region the liquid-water content is so low that the rapid passage of a small droplet freshly entering the cloud, or of a raindrop leaving it, is accompanied by very little growth. To illustrate this we may calculate the possible growth of a droplet of initial radius 30 μ in the lowest 0·6 km. of the cloud, when the updraught is 1 m/sec, the least with which we shall be concerned. If we assume that the value of E throughout is 0·7, then from Eq. (7) :

$$\frac{4}{0{\cdot}7}\int_{R_0}^{R_1}\frac{U-V}{V}\,dR=\int_0^{0{\cdot}6\text{ km}} m\,dz$$

or

$$\frac{4}{0{\cdot}7}\times\frac{90}{10}\int_{R_0}^{R_1}dR\simeq 50\,(R_1-R_0)=\int_0^{0{\cdot}6\text{ km}} m\,dz \quad . \quad . \quad (9)$$

The increase in the radius is thus about one-fiftieth of the integral on the right, which is found (Fig. 7, p. 412) to be $3{\cdot}5 \times 10^{-2}$ cm, *i.e.*, the increase is about 7 μ. In stronger updraughts the increase is proportionately reduced. A similar result is obtained if the average size of the cloud droplets is somewhat reduced by diminishing the water content of the cloud (lowering the base-temperature) or increasing their concentration, the effect being to raise the level above which E_1 generally exceeds

0·75 (to about 1 km in the coldest cloud). If therefore we assume any value for E in the lowest part of the cloud we introduce an error equivalent to an uncertainty in the initial value of R amounting to only a few microns, which can be regarded as quite unimportant. Throughout any cloud we may therefore consider E to be predominantly a function of z, or even a constant. In either instance we have from Eq. (8·2) :

$$\int_{R_0}^{R_1} \frac{U-V}{V}\, dR = 0$$

or

$$R_1 - R_0 = U \int_{R_0}^{R_1} \frac{dR}{V} \quad . \quad . \quad . \quad . \quad (10)$$

The integral on the right may be computed numerically and Fig. 4 shows it as a function of R_1 for a value of $R_0 = 20\ \mu$; the value used for V is that appropriate to a height of about 2 km, but for $R_1 > 1500\ \mu$, V has been taken to be 10 m/sec.* The curve in the figure may be used to solve Eq. (10) ; for any particular initial value R_0 of R a line is drawn from the corresponding point on the curve at a slope equal to U^{-1}, to meet the curve again at a point whose abscissa gives the value of R_1. In this way we determine the size of raindrops produced by droplets of initial radii of 20 to 40 μ solely as a function of the updraught speed, with the results shown in Fig. 5. The size of the raindrops increases with the speed of the updraught until at a critical speed between 3 and 4 m/sec drops of the maximum stable size

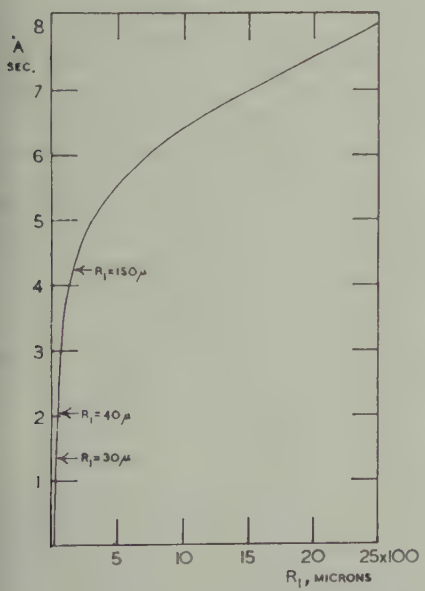


Figure 4. The variation of $\int_{R_0}^{R_1} \frac{dR}{V}$ (A) with R_1, for $R_0 = 20\ \mu$.

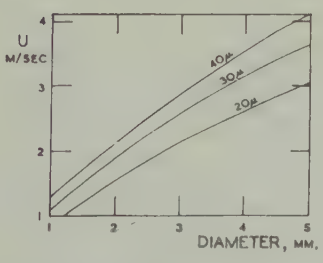


Figure 5. Diameters of raindrops grown from droplets of initial radius 20, 30, and 40 μ, as a function of the steady updraught U.

* For smaller radii, values of V were calculated using the drag-coefficients of spheres quoted by Langmuir and Blodgett (1946).

($R \simeq 2{\cdot}5$ mm) are produced. If the critical speed is exceeded drop-breaking occurs within the cloud. It is interesting to notice that the large raindrops develop in updraughts having speeds of only a third or a quarter of their fall-speed ; this has some bearing on fallacious deductions of updraught strength which in the past have been made from the size and fall-speed of hailstones.

The above results are based on the assumption of steady updraughts. Although the updraught frequently increases with height above the cloud base the change is not usually so rapid that we may not expect the results obtained from a mean updraught to be significant. We therefore continue with this assumption, reserving for a future consideration the effect of a variable updraught, which is now being determined by integrating the appropriate equations on the EDSAC (electronic delay storage automatic calculator) in the Cambridge University Mathematical Laboratory.

Fig. 5 also shows that the raindrop size increases with a decrease in the initial droplet radius ; this does not imply that the smallest droplets are the most efficient raindrop nuclei, for although their growth is potentially greater it may be accomplished only over a longer period which may extend beyond the life of a cumulus cloud. We have so far eliminated m from consideration, but we cannot do this if we wish to know whether the raindrops can actually be produced, for a low value of m might mean that insufficient time is available or that the growing droplet would be lifted to a great height where it might freeze, and therefore merely contribute to the development of the ice phase ; similarly the cloud might not be deep enough to allow unrestricted growth. We have thus now to consider whether raindrops can be formed at all.

5. A critical condition for the development of showers

Eq. (5) may be written

$$4\,dR = E\,m\,dS \quad . \quad . \quad . \quad . \quad (11)$$

where S is distance fallen relative to the air. Thus

$$S = 4\int \frac{dR}{\overline{E}\overline{m}} \quad . \quad . \quad . \quad . \quad (12)$$

If we write $E \simeq 1$ and $\overline{m} = 2 \times 10^{-6}$ g/cm^3 then

$$S \simeq 2 \times 10^6 R$$

and so a droplet which has grown by coalescence to a radius of 150 μ will have sunk about 300 m through the cloud air.

Now observation suggests that a droplet which has settled such a distance by the time the air in which it originated reaches the cloud summits has a good chance of escaping their subsequent evaporation. At this distance below the summits the updraught will not usually exceed the fall-speed (rather more than 1 m/sec) of a droplet of radius 150 μ, so that if it settles into a freshly-rising cloud mass it is likely still to recede from the summit level and continue its growth. This growth is rapid ; the substitution of the average values $\overline{E} = 1$, $\overline{V} = 300$ cm/sec and $\overline{m} = 2 \times 10^{-6}$ g/cm^3 into Eq. (5) shows that a raindrop ($R > 500\,\mu$) is produced within about 4 min. We may therefore justifiably regard a radius of about 150 μ

a critical radius ; if only droplets smaller than this have formed in air rising to e cloud summits they are likely to be evaporated quickly with the remainder of the roplet population ; on the other hand a droplet larger than this will not reach e summit level, but will tend to sink back into the freshly-rising air composing e bulk of the cloud, and there rapidly develop into a raindrop. This view is rengthened if we consider the survival of large droplets during the evaporation cloud masses containing them.

The evaporation of a droplet is described by Eq. (2) if δp is regarded as negative. we substitute dS/V for dt, and introduce a correction-coefficient C to take account the motion through the air, we obtain

$$RV\,dR = C\,k\,\delta p\,dS \quad . \quad . \quad . \quad . \quad (13)$$

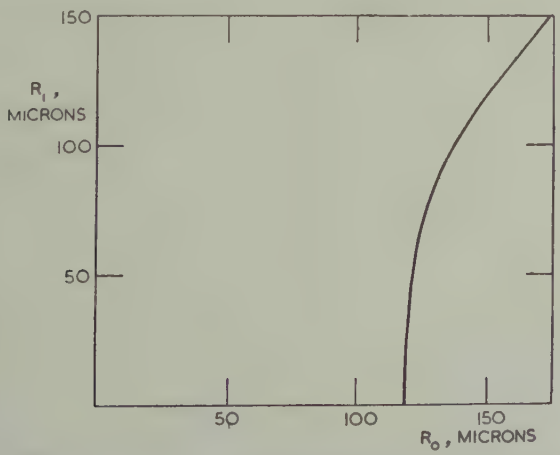
that

$$S = \int \frac{VR}{C\,k\,\delta p}\,dR \quad . \quad . \quad . \quad . \quad (14)$$

the relative humidity is assumed to be constant at 90 per cent., and if $k = 0{\cdot}325$ n^2/sec (appropriate to a height of about 3 km), then at a temperature of about 0°C, $\delta p \simeq -8 \times 10^{-8}$ g cm sec ; if now we put $S = 3 \times 10^4$ cm, *i. e.*, 300 m, we have

$$\int \frac{VR}{C}\,dR = -24 \times 10^{-4} \quad . \quad . \quad . \quad (15)$$

here $C = 1 + 0{\cdot}229\,(Re)^{1/2}$, Re being the Reynolds number (Frössling 1938). he integral may be evaluated numerically and used to find the radius R_1 of a droplet original radius R_0 after a fall of 300 m through air of 90 per cent R.H., at a tempera- re of about 0°C. The relation is shown in Fig. 6, which again reveals $R \simeq 150$ as a critical radius : a droplet of about this size can survive a fall of several hundred etres from evaporating summits with little change of radius, and can therefore ttle back into the cloud bulk and resume rapid growth, whereas a droplet somewhat aller completely evaporates.



ure 6. The radius R_1 of droplets, initial radius R_0, after falling 300 m through air of 90 per cent R.H., at a temperature of about 0°C.

We therefore define as a necessary condition for the development of a shower the growth of some droplets to a radius of about 150 μ during the time taken for air to rise from the cloud base to the summits. The condition may sometimes be insufficient when a wind shear causes the clouds to lean, so that any large droplets in the cloud summits tend to fall outside the cloud rather than back into it. From Eq. (7),

$$U\int_{R_0}^{150\,\mu}\frac{dR}{\overline{EV}}-\int_{R_0}^{150\,\mu}\frac{dR}{\overline{E}}=0{\cdot}25\int_{z_0}^{z_b}m\,dz \qquad . \qquad . \qquad (16)$$

where z_b is now about 300 m less than a critical summit-level z_c which must be reached if a shower is to develop. An approximate solution of this equation may be found by using a suitable mean value for E. From our previous consideration of the variation of E it appears that if we write $E = 0{\cdot}85$ the estimation of the terms on the left of the equation is subject to an error which at the most amounts to about 15 per cent and is likely to be much less. Even this error, however, hardly affects the value obtained for z_c because the integral on the right of the equation increases rapidly with height above the cloud base (Fig. 7). The greatest error amounts to about 90 m in 1000 m, and to about 250 m in 3000 m. Eq. (16) now becomes,

$$\int_{z_0}^{z_b}m\,dz=4{\cdot}7\left[U\int_{R_0}^{150\,\mu}\frac{dR}{\overline{V}}-1{\cdot}5\times10^{-2}+R_0\right] \qquad . \qquad . \qquad (17)$$

and this is readily used (by reference to Figs. 4 and 7) to find z_b, and hence the critical summit level, for various values of R_0 and U, with results which are shown in Fig. 8. It is clearly shown that the critical depth of cloud necessary for shower

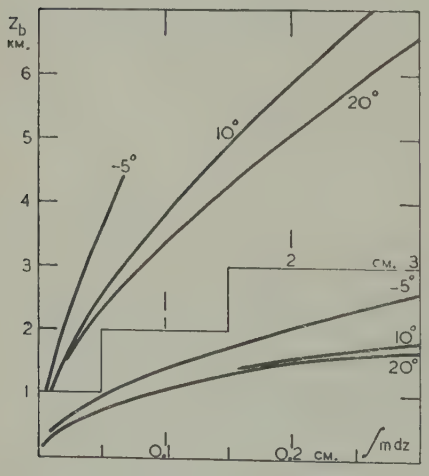


Figure 7. The depth of precipitable water, $\int m\,dz$, as a function of the height z_b above the base of clouds having base temperatures of −5, 10 and 20°C.

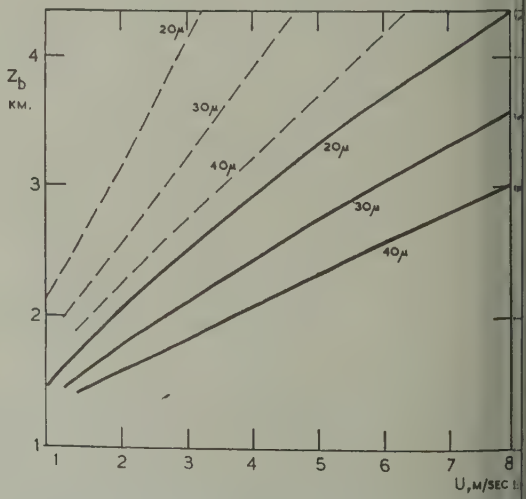


Figure 8. The variation of the minimum cloud depth z_c for shower development as a function of the updraught U, for initial droplet radii of 20 to 40 μ in clouds with base temperatures of −5°C (pecked lines) and 20°C.

roduction increases considerably with the speed of the updraught, and also is mewhat larger the colder the cloud base and the smaller the value of R_0. The arious curves are limited on the left by the condition that the final raindrop radius to exceed 500 μ, and it can be seen that this condition is always fulfilled even ith the weakest updraughts (1 to 2 m/sec) which are likely to occur in convective ouds of any considerable depth. The diagram shows that only moderate cloud epths of $1\frac{1}{2}$ to 3 or 4 km are required for the coalescence mechanism to produce a ower from droplets entering the clouds with radii of 20 or 30 μ. The time re-uired for the growth of a raindrop is found by adding a few minutes to the value btained by dividing the critical depth z_c by the updraught, and varies from about half n hour with $U = 1$ m/sec to about 20 min in the stronger updraughts. In order r a considerable shower to occur the life of the entire cloud must exceed these eriods by at least several minutes, and this is always likely except perhaps occasion-ly with the coldest clouds, in which the raindrop growth proceeds most slowly.

Because of the uncertainty concerning the appropriate values of E, and because ie contribution of continued condensation should also be considered, it is difficult assess the growth of droplets whose initial radius is less than 20 μ. However, ie relative fall-speeds of such droplets amongst the other cloud droplets become ery much smaller, and the critical cloud depth for raindrop growth may increase nsiderably on this account alone. Thus for an initial radius of 10 μ the increase ay be estimated to be about 1 to 2 km with $U = 1$ m/sec, and 2 to 6 km with $U = 3$ /sec, being least in clouds with high base-temperatures. Similar or rather reater increases are associated with the continued growth by coalescence of drops rmed by the chance combination of a pair of cloud droplets, in which instance ie initial relative fall-speed is only about half the fall-speed of the cloud droplets. ich substantial increases, particularly with moderate updraughts, indicate that ntinental air-mass cumulus, which is unlikely to contain droplets of $R_0 > 10$ μ, much more stable colloidally than the clouds of maritime air masses.

6. The rôle of coalescence in shower and thunderstorm production

(a) *Shower-cloud tops in relation to the 'freezing level'*

From the data contained in Figs. 7 and 8, Fig. 9 has been prepared. This agram shows the variation with base temperature of the minimum cloud depth kely to be associated with showers when the updraughts are weak (1 to 2 m/sec), id the critical depths for some other conditions. The isotherms for 0° and 20°C e also included ; although the diagram has been constructed for a cloud base at 0 mb, *i.e.*, about a kilometre, its features are not significantly changed if the base vel is varied by several hundred metres.

It is evident that if the temperature of the cloud base exceeds about 8°C showers ay fall from clouds whose summits do not reach the 0°C isotherm. If the base mperature approaches 20°C the shower-cloud tops are not supercooled even with rong updraughts (see, *e.g.*, the two uppermost curves of the diagram). On the her hand in the colder clouds moderate updraughts lift drops growing by coales-nce to levels where the temperature is considerably below 0°C ; they may then eeze in the presence of ice-nuclei. Large spray droplets which grow into raindrops

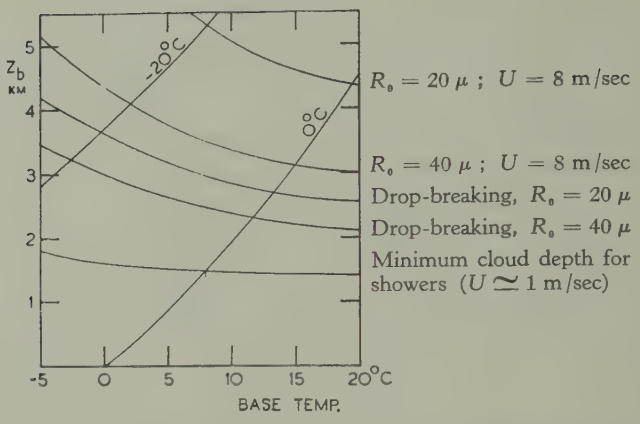


Figure 9. Occurrence of showers produced by the coalescence mechanism in relation to the temperature of the cloud base. The curves show the minimum cloud depths for the various conditions described on the right of the diagram.

may be expected to freeze about as readily as similar droplets of other natural waters, being about equally susceptible to contamination by a freezing nucleus. Alternatively the growing raindrops may become infected with ice nuclei from the air, or from the cloud droplets which they sweep up. In either instance the probability of the drop freezing at some temperature is indicated by the concentration of ice particles experimentally observed to be characteristic of this temperature when outdoor air is expanded to produce a cloud.

The volume A of air swept by a growing drop is given by

$$dA = E\pi R^2 V\, dt$$

$$= EmV\, dt\, .\, \frac{\pi R^2}{m}$$

or, from Eq. (5),

$$dA = \frac{4\pi R^2}{m} dR \quad . \quad . \quad . \quad . \quad (18)$$

Thus if $\bar{m}$ is a mean value of m,

$$A = \frac{4\pi}{3\bar{m}} \left[R^3\right]_{R_0}^{R_1} \quad . \quad . \quad . \quad . \quad (19)$$

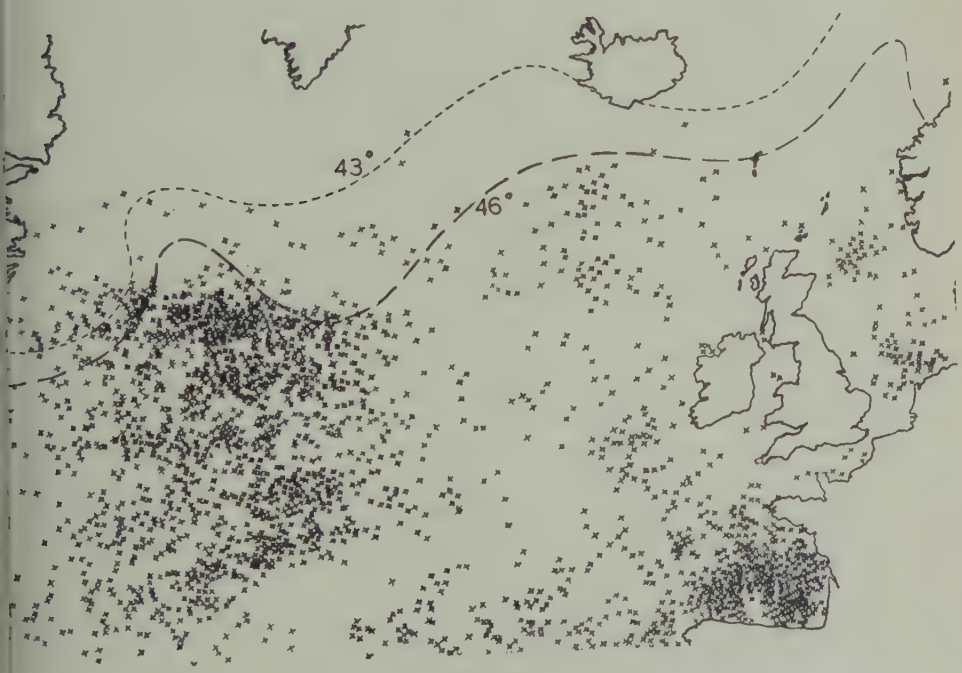
Now from Fig. 4 it appears that in a moderate updraught (3 m/sec) a droplet whose radius reaches 150 μ at some level grows to a radius of about 650 μ during its passage through higher regions, where it therefore sweeps a volume of air $A \simeq (4\pi/3\bar{m}) \times 270 \times 10^{-6}$ cm^3 or about half a litre if $\bar{m}$ is rather more than 2 g/m^3. In stronger updraughts the growth is increased and the volume swept rises to several litres. The available data (see, *e.g.*, Palmer 1949) suggest that such volumes are likely to contain an active ice nucleus once the temperature has fallen to about -20°C. If therefore the critical summit level exceeds the level of the -20°C isotherm then the raindrops growing by coalescence are likely to become frozen in the upper part

f the cloud, contributing only to the development of the ice phase. Fig. 9 shows iat this is always to be expected in moderately vigorous clouds if their base tempera-ure is below about 0°C.

(b) *The occurrence of raindrop breaking*

Fig. 5 shows that raindrops of the maximum stable size may be produced the updraught exceeds 3 to 4 m/sec. If, however, the base temperature of the louds falls below about 0°C it would appear from Fig 9 that the growing raindrops re lifted to levels where they are likely to become frozen before reaching the un-table size. It will be shown in a future paper that another mechanism of introducing arge drops into the cloud, the shedding of surplus water from wet hailstones (Ludlam 950), is also ineffective in these clouds. There is therefore reason to suppose hat raindrop breaking does not occur in such cold clouds.

This is an interesting conclusion, because a remarkable feature of maps showing he distribution of the thunderstorm sources of 'sferics' over the North Atlantic s the absence of sources in regions where the base temperature of the clouds falls elow about 0°C. Fig. 10, for example, shows all the sources located over the ocean y the Meteorological Office for two winter months ; except near Newfoundland, here the positions are subject to considerable error in longitude, the sources are onfined to regions where the sea temperature exceeds about 43°F, which with a



ure 10. 'Sferic' sources located by the Meteorological Office over the northern North Atlantic during Nov. and Dec., 1949, and two sea-temperature isotherms for the same period.

cloud base of about 2000 ft corresponds to a base temperature of 0°C. Recently Kuettner (1950) also found from an analysis of observations made on the Zugspitze that thunderstorms occurred only when the cloud base lay below the freezing level. Our considerations suggest that raindrop breaking may always play an active part in the separation of thunderstorm charges, and that diagrams such as Fig. 10 may provide valuable information in the theory of thunderstorm production. The breaking of raindrops can of course occur in the supercooled parts of the clouds, although it is unlikely where the temperature is below about — 20°C, and so the observed frequency with which this region is found as a site of charge separation cannot be interpreted as sure evidence that the ice phase is involved.

7. Conclusion

Some of the results of the present work may be summarised as follows :

(a) Small numbers of droplets introduced into the base of cumulus with radii of about 20 μ may readily lead to the development of showers.

(b) In unstable maritime air masses there are large sea-salt nuclei which can provide such droplets in the necessary concentrations. In continental air masses such effective agents may be absent, so that the clouds of these air masses may be colloidally more stable, perhaps producing showers only after the appearance of the ice phase.

(c) There is a minimum cloud depth for shower production by coalescence which increases with the speed of the updraught, and which decreases with a rise in the temperature of the cloud base and an increase in the size of the raindrop nuclei which enter the cloud base. The critical depth has a minimum value between about 1·5 and 2 km. If the cloud base temperature exceeds about 8°C showers may occur from clouds not reaching the freezing level.

(d) If the speed of the updraught exceeds 3 to 4 m/sec raindrop breaking may occur within the clouds. Strong updraughts, however, may lift growing raindrops to levels where they become frozen. With updraughts of 3 to 4 m/sec this is always probable in clouds having base temperatures below 0°C, and in such clouds, drop breaking cannot occur. There is evidence that thunder is not produced by these clouds.

(e) In many deep shower clouds the appearance of the ice phase may anticipate the coalescence mechanism in the production of precipitation, or the two mechanisms may proceed together. Over a wide range of conditions, however, the coalescence process may be of dominating importance, profoundly affecting the properties of clouds in respect of such phenomena as thunderstorm occurrence, aircraft icing, and response to different kinds of artificial stimulation. These aspects will be discussed in other papers.

Acknowledgment

I am indebted to the Director of the Meteorological Office for permission to publish Fig. 10.

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Drop-size distribution in cloud and fog

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SUMMARY

Experimental data relating to drop-size distribution in cloud and fog are examined and it is shown that in many cases the size distribution can be represented by the formula

$$1-F=\exp\left[-(x/a)^n\right]$$

where F is the fraction of liquid water in the air comprised by drops with diameter less than x. The constant a increases with the liquid water content of the cloud. It is not clear what governs the value of the constant n but a reasonable mean value is about 3·3.

It is shown that, if the drop-size distribution can be described by the equation quoted above the median volume diameter is the most suitable parameter for describing the average size of the cloud particles when the value of n is not known.

1. INTRODUCTION

In an earlier paper (Best 1950) the writer has shown that the size distribution of raindrops may be conveniently described by the formula

$$1-F=\exp\left[-(x/a)^n\right] \quad . \quad . \quad . \quad (1)$$

where, F = the fraction of liquid water in the air comprised by drops with diameter less than x, and a and n are constants. A purpose of the present paper is to show that a similar formula, with different values for the constants, may be used to describe drop-size distribution in cloud and fog.

Although the number of data available on drop-size distribution in cloud or fog is small many measurements have been made in recent years of 'average' or 'mean' drop-size in cloud. Unfortunately various writers have used different interpretations of the words average and mean so that it is not easy to compare their results. For example Bricard (1943) has quoted the arithmetical mean of the drop diameters whereas Lewis and Hoecker (1949) quote mean effective diameter – a parameter which appears to be equivalent to a diameter such that 50 per cent. of the water is comprised of smaller drops. If the shape of the size-distribution curve is known the relation between the various average diameters is easily determined. Accordingly this question will also be examined below on the assumption that Eq. (1) does describe the drop-size distribution.

2. THE DATA AVAILABLE

The data on drop-size distribution available to the writer were as follows :

(a) *Mazur's data*

During the period Jan. 1943 to May 1944 measurements of the size of cloud articles were made from an aircraft by Dr. J. Mazur for the Meteorological Office Mazur 1943). The instrument used, known as the Aircraft Impactor, consisted ssentially of a glass plate covered either with a uniform film of oil to trap impinging ater droplets or alternatively of magnesium oxide in which the impinging droplets ade craters. The exposed plate was subsequently examined visually through a icroscope and the drops or craters measured.

(b) *Diem's data*

Diem (1942 and 1948) has published two papers of which the second is essentially continuation of the first. He used three methods for measuring drop size, one which was very similar to that of Mazur and gave drop-size distribution. We hall not be concerned here with the other two methods. Diem presented his esults for drop-size distribution in the form of small block diagrams giving the ercentage number of drops falling between certain size limits. The work of reading alues from these small diagrams and working up the results was very laborious and as attempted only for those samples in which the number of drops measured xceeded 100. In his second paper Diem gives some volume-distribution curves, veraged over all his samples, for various cloud types irrespective of liquid-water ontent. These curves have also been used below though this involves some verlapping with the results obtained from the individual block diagrams.

(c) *Frith's data*

Frith (1951), using a method rather similar to Mazur's, has measured drop-size istributions in stratocumulus cloud and the present writer has been kindly granted ccess to the detached measurements.

(d) *Hagemann's data*

Hagemann (1936) using a capture-in-oil method has published measurements f drop-size distribution in fog. His measurements were made on 11 days and esults of individual samples are given in the form of small block diagrams. Rather naccountably Hagemann provides data about visibility and liquid-water content n the fog for only two out of the eleven days. In view of this no use has been ade of the small block diagrams relating to individual samples. Instead the ean curves for each day have been used.

3. Method of analysis and results

The aim of the analysis was to obtain values of F corresponding to various alues of the diameter x, having first grouped the results according to liquid water ontent W (g/m^3) and cloud type where practicable. The detailed procedure for ombining a group of samples differed slightly from one set of data to another cording to the form in which the data were available but these slight differences re unimportant. Having obtained corresponding values of F and x the results ere plotted in the form $\log \log_{10} [1/(1-F)]$ against $\log_{10} x$ and a few of these ots are shown in Fig. 1. If Eq. (1) can represent the data adequately each plot

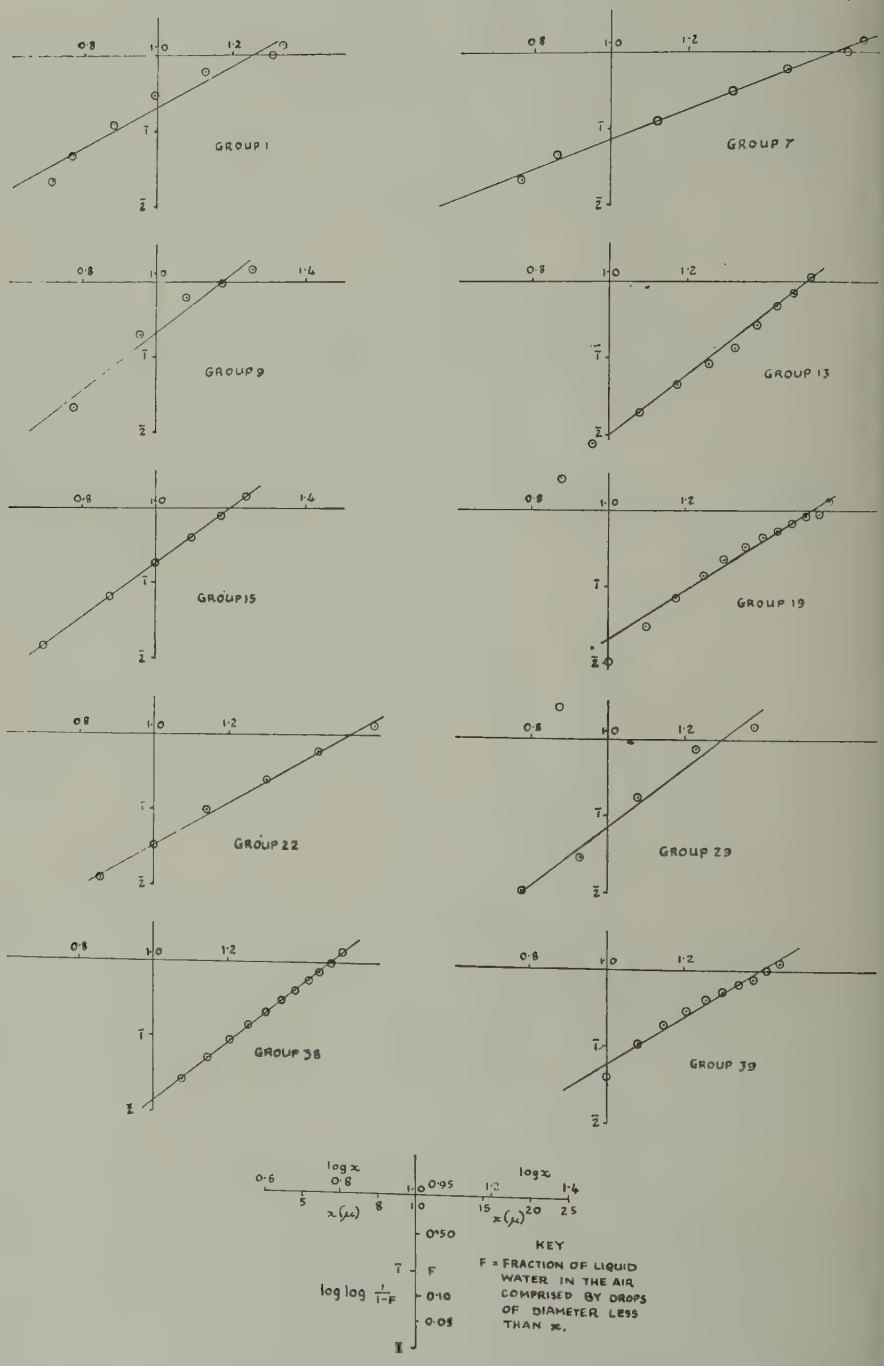

Figure 1. Fraction of volume of water as a function of drop size.

uld approximate to a straight line. The best straight line for each group of was computed from the coordinates of the points by the method of least squares. ept for Frith's data they were computed to give the best fit to those points for ch F lies between 0·1 and 0·9 and also those points for which F has the next e below 0·1 and the next value above 0·9. For Frith's data the criterion pted was to exclude those points for which F lies between 0·0 and 0·02 or between and 1·0. In practice the two criteria differed but little.

TABLE I. DATA ON DROP-SIZE DISTRIBUTION IN CLOUD AND FOG

ıp	Cloud Type	W (g/m³)	No. of samples	No. of clouds	No. of drops	n	a (μ)	σ
ur								
	Sc	0·07	14	4	6,940	2·67	13·3	0·135
	Sc	0·15	12	3	6,830	2·82	17·5	0·109
	Sc	0·35	12	3	9,075	3·04	22·6	0·082
	Cu	0·07	18	6	9,311	3·29	12·5	0·170
	Cu	0·13	5	3	2,863	4·86	14·0	0·068
	Cu	0·22	7	3	4,066	3·74	19·7	0·066
	St	0·13	10	3	5,068	1·92	25·3	0·042
	Sc	0·05	2	2	348	2·94	12·7	0·064
	Sc	0·15	3	3	359	3·82	12·1	0·132
	Sc	0·23	3	3	567	2·96	25·3	0·047
	Sc	0·48	2	2	646	4·90	17·8	0·049
	As	0·13	5	5	1,584	2·55	17·9	0·046
	Ns	0·30	4	3	599	4·05	26·7	0·017
	Cu	0·20	4	3	1,084	2·17	23.8	0·106
	Cu(1)	—	10	—	—	3·64	12·8	0·000
	As	—	31	—	—	3·76	16·9	0·052
	St	—	19	—	—	2·67	28·4	0·064
	Sc	—	26	—	—	3·32	12·9	0·000
	Cu(2)	—	8	—	—	3·21	26·1	0·071
	Ns	—	27	—	—	3·29	27·0	0·000
	Sc	0·47	5		2,586	2·52	24·3	0·082
	Sc	0·62	3	1	1,662	2·80	24·8	0·017
	Sc	0·83	4		2,520	2·50	29·1	0·092
	Sc	0·17	5		1,645	3·06	18·1	0·086
	Sc	0·33	4	1	1,663	2·29	29·4	0·077
	Sc	0·49	4		1,659	2·34	31·1	0·089
	Sc	0·07	2		359	3·73	13·3	0·061
	Sc	0·21	8	1	3,135	4·41	14·1	0·064
	Sc	0·41	3		1,900	3·83	16·1	0·119
mann								
	Fog	—	3	1	1.213	4·23	26·1	0·052
	Fog	—	9	1	1,950	3·25	37·7	0·047
	Fog	—	11	1	2,822	2·82	36·4	0·044
	Fog	—	6	1	1,755	3·15	34·7	0·052
	Fog	—	3	1	582	3·43	42·1	0·025
	Fog	—	1	1	420	3·35	34·3	0·030
	Fog	—	2	1	462	3·85	24·3	0·007
	Fog	—	3	1	847	3·35	25·0	0·024
	Fog	—	3	1	732	3·89	24·1	0·008
	Fog	—	17	1	7,913	3·09	19·3	0·067
	Fog	—	9	1	1,446	3·27	28·1	0·017

From the constants of the straight lines the values of n and a can be determined. These are given in Table I together with information indicating the classification of the group of data and also the number of samples, clouds and drop measurements contributing to the various groups. There are a few gaps in the table where the original data do not permit computation of the corresponding quantities.

It should be pointed out that there is some overlapping between groups 8 to 14 and groups 15 to 20. The former set are based upon some of Diem's block diagrams for individual samples ; the latter upon Diem's mean curves for cloud types and presumably include all samples.

Table I includes a quantity σ. This is the root mean square of the difference between $\log \log_{10} [1/(1-F)]$ and $n \log_{10} (x/a) - 0{\cdot}362$, *i.e.* it is the standard deviation of the experimental points about the line of best fit. This quantity thus provides a criterion of the goodness of fit of the computed straight lines. The groups of data plotted in Fig. 1 have been selected to represent the best and worst groups in each of the five sets.

Examination of Table I shows that there is no systematic variation of the distributive index n either with cloud type or with liquid-water content. The mean of the 40 values for n is 3·27 with a standard deviation of 0·68.

Turning now to the values of the scale diameter a it seems that there is no significant difference between the values for different cloud types but there does appear to be a connection between the values of a and W. For the 23 groups for which W is quoted in Table I $\log_{10} a$ has been plotted against $\log_{10} W$. The figure is not reproduced here but the points showed a general increase of $\log_{10} W$ with $\log_{10} a$, consistent with the relation $W = 1{\cdot}1 \times 10^{-3}\, a^{1{\cdot}79}$. The scatter of the points however was rather large, the standard deviation of $\log_{10} W$ about the line of best fit being 0·222. This corresponds to an uncertainty factor in the value of W, deduced from the scale diameter a by means of the relation quoted above, of about 1·7.

The main purpose of Table I is to show that Eq. (1) adequately represents the observations. That this is so can be seen from the figure and from the tabulated values of σ. Of the 29 values of σ obtained from cloud data, 22 are less than 0·09 and all the values obtained from Hagemann's fog data are less than 0·07. The mean of the 40 values of σ is 0·060. This value of σ corresponds to the difference between 0·100 and 0·114, 0·500 and 0·549 or 0·900 and 0·929 for F.

4. The average size of cloud drops

Having established that Eq. (1) can adequately represesent the distribution of drop sizes in a natural cloud we can now consider the relation between the various parameters which may be used to represent the average size of cloud drops.

We define five parameters as follows :

D_m The mean diameter, *i.e.* the sum of all the drop diameters divided by the number of drops.

D_d The mode diameter, *i.e.* the diameter corresponding to the maximum number of drops.

D_v The mean volume diameter, *i.e.* the diameter of a drop whose volume is equal to the average drop volume.

D_p The predominant diameter, *i.e.* the diameter corresponding to the maximum volume of water.

D_n The median volume diameter, *i.e.* the diameter of a drop such that half the water is comprised by larger drops.

any experimental measurements of drop size in cloud there is a minimum drop-size which the measuring apparatus will respond. By definition D_m and D_v depend on the number of drops measured and we might therefore expect that the value D_m and D_v would depend upon the minimum measurable diameter d. Since . (1) is dimensionless it is to be expected that d will be significant only in the form a and that the five average diameters should be expressible as fractions of a, e fractions depending upon n and, for D_m and D_v, upon d/a. This is in fact the se and in Table II the numerical values of D/a are given for various values of and d/a. The method of computation is given in the Appendix.

TABLE II. VALUES OF THE FIVE MEAN DIAMETERS

	any value		0·05		0·10		0·20	
D_n/a	D_p/a	D_d/a	D_m/a	D_v/a	D_m/a	D_v/a	D_m/a	D_v/a
0·783	0·481	—	0·112	0·228	0·201	0·331	0·282	0·494
0·833	0·707	—	0·148	0·301	0·243	0·391	0·393	0·527
0·864	0·815	—	0·210	0·393	0·301	0·464	0·396	0·551
0·885	0·874	—	0·301	0·492	0·376	0·541	0·488	0·617
0·901	0·908	—	0·410	0·585	0·460	0·613	0·544	0·664
0·912	0·931	—	0·516	0·663	0·543	0·677	0·599	0·707
0·922	0·946	0·614	0·605	0·723	0·617	0·729	0·650	0·746
0·929	0·956	0·725	0·672	0·767	0·678	0·770	0·697	0·779

ere are no values for D_d when $n \leqslant 4$ because the number-distribution curve then s no mode.

We see from Table II that if n is not known D_m and D_v are quite unsuitable specifying the mean drop size not only because they vary with d but also because ey vary considerably with n. In this respect D_p is a more suitable parameter t D_n is even better.

It is clear from Table II that there is a value of n for which D_n and D_p are equal d it is shown in the Appendix that this value of n is 3·26. It is a happy coincidence at this value of n is so close to the mean of the 40 values tabulated in Table I.

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Appendix

Computation of the average size of cloud drops

We suppose that at diameters greater than the minimum measurable size the drop-size distribution is described by Eq. (1).

The volume of water comprised by drops with diameter between x and $x + \Delta x$ is

$$W\frac{\partial F}{\partial x}\Delta x = W\frac{nx^{n-1}}{a^n}\exp\{-(x/a)^n\}\Delta x \quad . \quad . \quad . \quad (2)$$

and the number of drops with diameter between x and $x + \Delta x$ is

$$\frac{6W}{\pi x^3}\frac{\partial F}{\partial x}\Delta x = \frac{6W}{\pi}\frac{nx^{n-4}}{a^n}\exp\{-(x/a)^n\}\Delta x \quad . \quad . \quad (3)$$

Now D_p and D_d are the values of x which give maximum values to the right hand sides of Eq. (2) and (3) respectively. We thus get

$$D_p = a\left(1-\frac{1}{n}\right)^{1/n} \quad . \quad . \quad . \quad . \quad (4)$$

$$D_d = a\left(1-\frac{4}{n}\right)^{1/n} \quad . \quad . \quad . \quad . \quad (5)$$

We obtain D_n as the solution of the equation

$$\frac{1}{2} = \exp\{-(x/a)^n\}$$

or

$$D_n = a(\log 2)^{1/n} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

If $D_n = D_p$ we have

$$(\log 2)^{1/n} = \left(1-\frac{1}{n}\right)^{1/n}$$

leading to

$$n = 3{\cdot}26 \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Turning now to the computation of D_m and D_v we have

$$D_m = \frac{\dfrac{6Wn}{\pi a^n}\displaystyle\int_d^\infty x^{n-4}\exp\{-(x/a)^n\}x\,dx}{\dfrac{6Wn}{\pi a^n}\displaystyle\int_d^\infty x^{n-4}\exp\{-(x/a)^n\}\,dx}$$

$$\frac{\pi D_v^3}{6} = \frac{W}{\frac{6Wn}{\pi a^n}\int_d^\infty x^{n-4}\exp\{-(x/a)^n\}\,dx}$$

ere d is the diameter of the smallest measurable drop. Putting $(x/a)^n = t$ d $d/a = h$ we get

$$\frac{D_m}{a} = \frac{X_2}{X_3} \qquad \frac{D_v}{a} = (1/X_3)^{1/3} \quad . \quad . \quad . \quad (8)$$

ere

$$X_2 = \int_{h^n}^\infty e^{-t}\,t^{-2/n}\,dt, \qquad X_3 = \int_{h^n}^\infty e^{-t}\,t^{-3/n}\,dt$$

first sight it appears that the functions X_2 and X_3 should be easily obtainable m tables of the Incomplete Gamma function. However we have seen that a likely to have a value from 10 to 30 μ and experimental measurements appear e reliable down to $d = 1$ or 2 μ. Thus h may have a value of the order 0·05 and only tables known to the writer do not deal with such small values of the limit. order to ensure that significant quantities were not neglected the formulae for and X_3 were put into the following form by repeated integrations :

$$X_2 = -A_2(h^{n-2} + B_2) + C_2\,[1 - I(u_2, p_2)] \quad . \quad . \quad (9)$$

$$X_3 = -A_3(h^{n-3} + B_3) + C_3\,[1 - I(u_3, p_3)] \quad . \quad . \quad (10)$$

ere

$$A_2 = \frac{e^{-h^n}}{1-2/n} \qquad A_3 = \frac{e^{-h^n}}{1-3/n}$$

$$B_2 = \frac{h^{2n-2}}{2-2/n} \qquad B_3 = \frac{h^{2n-3}}{2-3/n}$$

$$C_2 = \frac{\Gamma(3-2/n)}{(1-2/n)(2-2/n)} \qquad C_3 = \frac{\Gamma(3-3/n)}{(1-3/n)(2-3/n)}$$

$$u_2 = \frac{h^n}{(3-2/n)^{\frac{1}{2}}} \qquad u_3 = \frac{h^n}{(3-3/n)^{\frac{1}{2}}}$$

$$p_2 = 2-2/n \qquad p_3 = 2-3/n$$

$$I(u,p) = \frac{\int_0^{u(p+1)^{\frac{1}{2}}} e^{-t}\,t^p\,dt}{\Gamma(p+1)}$$

e advantage of Eqs. (9) and (10) is that the function I is tabulated in Pearson's le of the Incomplete Gamma function and the values of the parameters are such t, although I cannot be evaluated from the tables the latter show that the values with which we are concerned are small (less than 0·02) in the cases considered npared with unity and so can be neglected.

Eq. (9) fails when $n = 2$ since A_2, B_2 and C_2 become infinite. For similar reasons Eq. (10) also fails when $n = 1{\cdot}5$ or 3. It is clear that when $n = 2$ or 3 respectively

$$X_2 = \int_{h^2}^{\infty} \frac{e^{-t}}{t}\, dt = ei(h^2)$$

$$X_3 = \int_{h^3}^{\infty} \frac{e^{-t}}{t}\, dt = ei(h^3)$$

and to evaluate the exponential integral in these cases we use the expansion (*Mathematical Physics*, H. and B. S. Jeffreys, 1st Edn., p. 441)

$$ei(y) = -\gamma - \log y + y - \frac{y^2}{2.2!} + \frac{y^3}{3.3!} - \quad . \quad . \quad .$$

To compute X_3 when $n = 1{\cdot}5$ we use

$$X_3(n = 1{\cdot}5) = \frac{e^{-h^{1\cdot5}}}{h^{1\cdot5}} - ei(h^{1\cdot5})$$

551.510.53 : 551.524.7 : 551.508.822

The apparent diurnal temperature variation in the lower stratosphere

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SUMMARY

Two years of upper-air data from the four daily radiosonde ascents from five stations in the British Isles are analysed for the pressure levels 150 mb, 100 mb and 80 mb – some 14,000 ascents in all.

The apparent diurnal temperature changes are discussed and the need is shown for further experimental evidence concerning the reality of the measured high-level daylight temperatures. The observed daylight heating is too great to be caused by the absorption of radiation and must either be dynamical in origin or due to radiation error of the thermometer element in the radiosonde. From 0048, the times of ascent are suitable for computing the *nocturnal* cooling throughout the year, independent of radiation error. The temperature falls during the night by about 0·05°C hr^{-1} at these levels, the sort of change to be expected if radiative processes predominate.

The apparent rate of change of temperature closely following sunrise and just preceding sunset is about ten times more rapid than the nocturnal cooling.

It is concluded that the amplitude and rate of the apparent variations of temperature measured in daylight ascents should be treated with reserve until further experimental evidence as to their reality is available.

1. INTRODUCTION

It has long been accepted that the striking differences which exist between the vertical temperature distributions in the troposphere and in the stratosphere may, to a first approximation, be accounted for by assuming that, in the troposphere the temperature lapse rate is controlled for the most part by convective processes, whereas in the stratosphere convection is small and the temperature is governed mainly by absorbed and emitted radiation. Further, in spite of the complexities in the relevant absorption spectra and of difficulties in measuring directly the amount and vertical distribution of the three main absorbing gases (water vapour, carbon dioxide and ozone) in the lower stratosphere it is nevertheless possible to make a more or less quantitative estimate of the probable temperature distribution, assuming radiative equilibrium and reasonable amounts of the absorbing gases (Dobson 1946, Gowan 1947). Mean temperatures computed in this way agree fairly well, considering the difficulty of accurate computation, with those actually measured in radiosonde ascents.

Recently, however, much attention has been directed to *diurnal variations* of temperature in the lower stratosphere measured in radiosonde ascents and these, if real, appear to be considerably too large to be explained by radiative heating.

Wulf, Hodge and Obloy (1946) in the United States believe in 'the reality of a considerable part at least of the difference of temperature of the air between day and night over the first 10 to 20 km of altitude as shown by mean radiosonde data.' At 18 km, values as high as 4 to 5°K in summer, diminishing to the order of 2°K in winter, between 11 and 23 hr LMT are reported as the measured mean daily range. These figures give the measured diurnal temperature range at a fixed *height* and are slightly less readily interpreted than figures computed for a fixed *pressure level* ; it is however apparent that the measured diurnal range is much greater than anything predicted from theories of radiative equilibrium. (The

ascents quoted are not exactly comparable with those in Great Britain, for in the former the tropopause is often at 16 km).

Gowan, Möller, Pendorff and other authors calculate that diurnal temperature variations due to radiative processes at heights between 15 and 20 km are only of the order of tenths of 1°K, assuming radiative equilibrium.

In England, Piegsa (1947), analysing about four years (1942-1946) of daily radiosonde observations made at Liverpool finds for a given pressure level an average range of diurnal temperature variation at the greatest heights available from routine radiosonde ascents (50-60 mb) of approximately 5°K, decreasing to about 3°K at 100 mb and to 2°K at 150 mb and concludes that ' at least one half or more of this measured diurnal temperature range is real and cannot be ascribed to errors caused by solar radiation heating directly the temperature-sensitive element of the radiosonde.'

The difficulty in interpreting these radiosonde results lies in differentiating between true heating of the free air and temperature error due to insolation of the thermometer element. The ascents analysed by Piegsa took place at 00, 06, 12 and 18 hr GMT each day.

The same programme of radiosonde ascents was continued in 1947 and the writer has analysed the ascents for that year for five widely separated stations in the British Isles, hoping thereby to reduce the chance of new air masses passing simultaneously over all the stations, since temperature variations due to horizontal transport of air are much larger than true diurnal changes. The above times of ascent are not ideal for this purpose, for only in the winter months is there more than one ascent per day which takes place wholly in darkness, *i.e.* when solar-insolation errors cannot exist.

In 1948 the times of ascent were changed to 03, 09, 15 and 21 hr GMT and these have the advantage that the 03 and 21 hr ascents take place wholly in darkness throughout the year. (See, however, footnote in Section 2). These ascents have also been analysed for the same five stations. The instrument used is the Kew radiosonde which has been described by Dymond (1947), who also discusses the probable temperature errors of the instrument and quotes earlier investigations of this problem reported to the Meteorological Research Committee.

2. Treatment of the data and results

Temperatures measured in the four daily ascents from five stations in the British Isles were taken from the *Daily Weather Reports* (upper-air section) issued by the Meteorological Office.

The five stations investigated were :

Lerwick	60°N.	1°W.
Larkhill	51·5°N.	2°W.
Downham Market	52·5°N.	0·5°E.
Aldergrove	54·5°N.	6°W.
Penzance	50°N.	5·5°W.

Routine radiosonde ascents normally begin 1 hr before the nominal times and the balloon usually bursts after about 45-50 min. The rate of ascent is about 1,200 ft min^{-1} but decreases to about 800 or 900 ft min^{-1} at the top of the ascent.

most cases the balloon is launched promptly but on a small percentage of casions there is a delay which may very rarely be as much as one hour. The *aily Weather Report* does not distinguish between late and prompt ascents.

For 1947, the temperature differences between the 06, 12 and 18 hr ascents nd the mean of the previous and following midnight ascents were computed for ch day at the pressure levels 150 mb, 100 mb, and 80 mb. These diurnal differences ere meaned over the month.

Seasonal means for spring (February, March, April), summer (May, June, ly), autumn (August, September, October), winter (November, December, nuary) were also computed ; the seasons being taken in this way to correspond the march of the altitude of the sun on which insolation errors might possibly epend. The results for 1947 are shown in Tables I and II.

For 1948, the temperature difference of the 09 and 15 hr ascents from the ean of the 03 and 21 hr ascents was computed for each day at the same three ressure levels and the same five stations. The temperature difference between e 21 and 03 hr ascents was also calculated ; the latter giving a figure for the cooling uring those six hours of the night, independently of insolation errors.* The daily fferences were treated exactly as for 1947 and the results appear in Tables III and IV.

3. Discussion of the results

(*a*) *The nocturnal cooling*

1947. The times of ascent suffer from the disadvantage that only in winter two of the daily ascents take place entirely in darkness.

Here the temperature difference $T_{18} - T_{06}$ is of the order of 0·1°K at all levels, dicating how small is the cooling during the night. Unfortunately these figures e complicated by an apparent increase in temperature at midnight (compared ith 18 hr) of about 0·5°K at 80 mb, the corresponding value decreasing to about 1°K at 150 mb. However, Piegsa found with a similar probable error that the lue $T_{18} - T_{00}$ for 1942-1946 varied from $-0{\cdot}25$°K at 150 mb to $+0{\cdot}5$°K at 80 b ; in exactly the opposite direction to that found above. The apparent tempera- re rise between 18 hr and midnight for the one year analysed here seems unlikely erefore to be significant compared with the general fall through the night. To be finite on this latter point one would probably need a series of nocturnal ascents one-or two-hourly intervals.

1948. Here the 03 and 21 hr ascents take place throughout the year, with the ception noted above, in darkness. An examination of Table IV will show that om the data available one can detect no appreciable seasonal effect in the fall of mperature from 21 to 03 hours as measured by the radiosondes. The fall in mperature during these six hours of the night increases from about 0·25°K at 0 mb to somewhat less than 0·5°K at 80 mb.

We may conclude that the evidence points to a nocturnal cooling of the order

* From late May to mid-July, the 03 and 21 hr ascents at Lerwick probably take place in sunlight. However, for y low sun, the apparent temperature changes, whether real or due to radiation error, are small and it was thought advisable retain the Lerwick results for this period, to gain the advantages of having another station. A similar argument applies he December 09 and 15 hr ascents which may take place in darkness at Lerwick. No significant difference is found for se months between Lerwick and the other stations. In addition there is always an uncertainty in the time at which the rument reaches a given pressure level which might possibly be as large as one hour.

TABLE I. 1947 MONTHLY MEANS OF THE MEASURED DIURNAL TEMPERATURE VARIATIONS (°K)

	$T_{12} - T_{00}$			$T_{06} - T_{00}$			$T_{18} - T_{00}$			$T_{18} - T_{06}$		
Month	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb
Jan.	+1·8	+ 1·8	+ 1·8	− 0·6	− 0·05	+ 0·3	− 0·8	− 0·2	+ 0·1	− 0·3	− 0·1	− 0·2
Feb.	+ 3·3*	+ 2·9	+ 2·0	+ 0·1*	+ 0·2	− 0·2	+ 0·2	+ 0·2	+ 0·5	+ 0·1*	− 0·1	+ 0·6
Mar.	+ 4·1*	+ 2·7	+ 2·2	+ 0·1*	− 0·2	− 0·2	+ 1·0*	+ 1·2	+ 1·4	+ 0·9*	+ 1·3	+ 1·6
Apr.	+ 4·2†	+ 2·3	+ 2·2	+ 1·4†	+ 0·6	+ 1·0	+ 2·8†	+ 1·6	+ 1·9	+ 1 5†	+ 1·0	+ 0·9
May	+ 4·9	+ 3·4	+ 2·6	+ 2·1*	+ 1·8	+ 1·5	+ 3·3	+ 2·1	+ 1·7	+ 1·2*	+ 0·3	+ 0·2
June	+ 4·2*	+ 3·5	+ 2·7	+ 2·8*	+ 2·6	+ 2·0	+ 3·2*	+ 2·6	+ 2·1	+ 0·4*	0·0	+ 0·05
July	+ 4·4	+ 2·6	+ 2·6	+ 2·2	+ 1·4	+ 1·6	+ 2·6	+ 1·4	+ 1·4	+ 0·4	+ 0·05	− 0·3
Aug.	+ 2·5	+ 2·6	+ 1·9	+ 0·9	+ 1·2	+ 0·9	+ 1·7	+ 1·6	+ 1·7	+ 0·8	+ 0·4	+ 0·8
Sept.	+ 3·2	+ 2·6	+ 2·0	+ 0·7	+ 1·2	+ 0·5	+ 1·3	+ 1·7	+ 0·9	+ 0·7	+ 0·4	+ 0·4
Oct.	+ 2·1	+ 2·1	+ 1·2	− 0·8	− 0·6	− 0·5	− 0·2	− 0·4	− 0·4	+ 0·6	+ 0·1	+ 0·2
Nov.	+ 2·4	+ 1·7	+ 1·3	− 0·5	− 0·3	− 0·4	− 0·1	− 0·3	− 0·05	+ 0·4	+ 0·1	+ 0·3
Dec.	+ 2·5	+ 2·6	+ 1·6	− 0·7	− 0·4	− 0·4	− 0·8	− 0·4	− 0·4	− 0·1	− 0·05	+ 0·1

For 80 mb, standard errors of the mean lie between ± 1°K and ± 0·5°K, and standard deviations from the mean between ± 2·5°K and ± 4°K.

For 100 mb, standard errors of the mean lie between ± 0·3°K and ± 0·7°K, and standard deviations from the mean between ± 2·5°K and ± 4°K.

For 150 mb, standard errors of the mean lie between ± 0·2°K and ± 0·3°K, and standard deviations from the mean between ± 2·5°K and ± 3·5°K.

* Results available for two of the five stations only. † Results available for one of the five stations only.

TABLE II. 1947 SEASONAL MEANS OF THE MEASURED DIURNAL TEMPERATURE VARIATIONS (°K)

	$T_{12} - T_{00}$			$T_{06} - T_{00}$			$T_{18} - T_{00}$			$T_{18} - T_{06}$		
	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb
Spring	+ 3·9	+ 2·6	+ 2·1	+ 0·6	+ 0·2	+ 0·2	+ 1·3	+ 0·9	+ 1·2	+ 0·8	+ 0·8	+ 1·0
Summer	+ 4·5	+ 3·2	+ 2·6	+ 2·3	+ 1·9	+ 1·7	+ 3·0	+ 2·0	+ 1·7	+ 1·3	+ 0·1	0·0
Autumn	+ 2·6	+ 2·4	+ 1·7	+ 0·3	+ 0·6	+ 0·3	+ 0·9	+ 0·9	+ 0·8	+ 0·7	+ 0·3	+ 0·4
Winter	+ 2·2	+ 2·0	+ 1·6	− 0·6	− 0·3	− 0·1	− 0·6	− 0·3	− 0·1	+ 0·1	0·0	0·0

Standard errors of seasonal means ± 0·3°K to ± 0·5°K.

Month	$T_{03}-T_{21}$			$T_{09}-(T_{03}+T_{21})/2$			$T_{15}-(T_{03}+T_{21})/2$			$T_{15}-T_{03}$		
	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb
Jan.	− 0·1	− 0·2	− 0·2	+ 2·5	+ 2·1	+ 1·2	+ 2·4	+ 1·3	+ 0·9	− 0·1	− 0·8	− 0·3
Feb.	− 0·4	− 0·3	− 0·0	+ 2·3	+ 1·6	+ 1·2	+ 1·6	+ 2·0	+ 1·4	− 0·7	+ 0·4	+ 0·2
Mar.	− 0·2	− 0·1	− 0·2	+ 2·6	+ 2·0	+ 1·7	+ 3·2	+ 2·6	+ 1·8	+ 0·6	+ 0·5	+ 0·1
Apr.	− 0·3	− 0·2	+ 0·1	+ 3·3	+ 2·7	+ 1·6	+ 3·3	+ 2·7	+ 1·5	− 0·0	− 0·0	− 0·1
May	− 0·6	− 0·3	− 0·9	+ 3·4	+ 2·5	+ 1·8	+ 3·7	+ 2·9	+ 1·9	+ 0·3	+ 0·4	+ 0·1
June	− 1·0	− 0·9	− 0·2	+ 3·7	+ 3·2	+ 2·2	+ 4·2	+ 3·4	+ 1·8	+ 0·4	+ 0·2	− 0·4
July	+ 0·6	+ 0·3	+ 0·6	+ 2·6	+ 2·4	+ 1·4	+ 2·8	+ 2·5	+ 1·3	+ 0·1	− 0·1	− 0·0
Aug.	− 0·4	− 0·3	+ 0·1	+ 3·5	+ 2·9	+ 1·9	+ 3·3	+ 2·8	+ 1·8	− 0·2	− 0·0	− 0·1
Sept.	+ 0·1	− 0·2	+ 0·2	+ 2·7	+ 2·8	+ 1·5	+ 3·1	+ 2·8	+ 1·5	+ 0·4	+ 0·0	− 0·0
Oct.	− 0·2	− 0·4	− 0·5	+ 2·7	+ 2·1	+ 1·4	+ 2·4	+ 2·0	+ 1·7	− 0·3	− 0·1	+ 0·3
Nov.	− 0·3	+ 0·3	− 0·1	+ 2·1	+1·5	+ 0·4	+ 1·9	+ 1·4	+ 0·4	− 0·2	− 0·0	− 0·0
Dec.	− 1·1	− 0·9	− 0·9	+ 1·3	+ 0·9	+ 0·6	+ 2·4	+ 2·1	+ 2·0	+ 1·0	+ 1·1	+ 1·4

For 80 mb, standard errors of the mean lie between ± 1°K and ± 0·5°K, and standard deviations from the mean between ± 2·5°K and ± 4°K.

For 100 mb, standard errors of the mean lie between ± 0·3°K and ± 0·7°K, and standard deviations from the mean between ± 2·5°K and ± 4°K.

For 150 mb, standard errors of the mean lie between ± 0·3°K and ± 0·2°K, and standard deviations from the mean between ± 2·5°K and ± 3·5°K.

TABLE IV. 1948 SEASONAL MEANS OF THE MEASURED DIURNAL TEMPERATURE VARIATIONS (°K)

	$T_{03}-T_{21}$			$T_{09}-(T_{03}+T_{21})/2$			$T_{15}-(T_{03}+T_{21})/2$			$T_{15}-T_{03}$		
	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb	80 mb	100 mb	150 mb
Spring	− 0·3	− 0·2	− 0·0	+ 2·7	+ 2·1	+ 1·5	+ 2·7	+ 2·4	+ 1·6	0·0	+ 0·3	+ 0·0
Summer	− 0·3	− 0·3	− 0·2	+ 3·2	+ 2·7	+ 1·8	+ 3·5	+ 2·9	+ 1·7	+ 0·3	+ 0·2	− 0·1
Autumn	− 0·2	− 0·3	− 0·1	+ 3·0	+ 2·6	+ 1·6	+ 2·9	+ 2·5	+ 1·6	− 0·0	− 0·0	+ 0·0
Winter	− 0·5	− 0·3	− 0·4	+ 2·0	+ 1·5	+ 0·7	+ 2·2	+ 1·6	+ 1·1	+ 0·2	+ 0·0	+ 0·3

Standard errors of seasonal means ± 0·3°K to ± 0·5°K.

of 0·05°K per hour or 0·5°K for the whole night. An analysis of many more observations will be necessary to detect with certainty whether this rate of cooling is dependent on the season.

Though qualitatively in agreement with Gowan's figures, the cooling is nevertheless some five times greater than he predicts for the 15 to 20 km level, but not significantly different from the figures given for the 20 to 25 km level. Elsasser however estimates a possible cooling of the order of 1°K day^{-1}. On the whole the measured nocturnal cooling is of the order of magnitude expected when radiative control is largely predominant.

(*b*) *Apparent heating during the day*

The main characteristics may be summarized as follows :

(i) The mean midday-midnight temperature differences measured for the layer 150-80 mb vary from *c.* 1·75°K in winter through *c.* 2·5°K in spring and autumn to 3°K in summer. This apparent heating is generally in good agreement with that found by Piegsa except that his figures for 80 mb tend to be higher. He also gives figures for 50 mb with larger temperature differences, up to 5°K. The data for 1947/48 show that ascents to 50 mb are far too few for a reliable seasonal mean to be calculated. The criterion in the present case was that at least five values of $T_{00}-T_{12}$ for each station must be available in any one month, giving a minimum of twenty-five ascents in all for each month.

(ii) The apparent diurnal range at 80 mb is of the order of 1½ to 2 times the range at 150 mb, *i.e.* at these levels the apparent diurnal range is roughly inversely proportional to the pressure.

(iii) The apparent rate of increase of temperature from sunrise to noon is of the order of 0·4°K hr^{-1} followed by a similar decrease in temperature from noon to sunset.

(iv) For the same available solar energy, to a first approximation, *i.e.* for spring and autumn, or for the same solar height, the apparent diurnal range is similar. (Piegsa found the autumn values to be near those for summer with spring considerably lower. This is not repeated in the analysis for 1947, 1948 ; if anything the spring values appear to be *c.* 0·5°K higher).

(v) The asymmetry about noon of the apparent heating is not very marked compared with the total apparent daily temperature range. T_{15} is about 0·1°K greater than T_{09} ; also T_{18} is 0·3° to 0·5°K higher than T_{06}.

(*c*) *Remarks*

Calculations of the solar-energy flux make it clear that the measured daylight increases in temperature and in particular the large rate of increase in temperature closely following sunrise cannot be accounted for by the absorption of solar radiation by the atmosphere. Nor is it likely that the rapid decrease in temperature preceding sunset can be explained by radiative processes.

There are two alternatives remaining. First, the major part of the observed midnight-midday temperature range may be a radiation error due to solar radiation falling directly on the thermometer element. The slowing down of the balloon at the top of the ascent, presumably because of leakage, will accentuate the reduced ventilation present in any case, because of reduced air pressure. Alternatively, the

perature changes may be due to dynamical causes ; this would not be inconsistent h the observed increase in the daytime temperature range with decreasing pressure.

The dynamical explanation is not without its difficulties. For instance one ld imagine, given an isothermal stratosphere, that the air at 100 mb at noon been at a lower pressure $(100 - p)$ mb in the early morning and had been ted by compression. This would involve an outflow of air below the level con- ered, to compensate the inflow above producing the pressure increase, there ng no diurnal pressure variation of the magnitude or phase here required at und level. An inflow of about 4 mb of air above the level considered (accom- ied by an outflow of the same amount of air below) would produce a tempera- e change of about 2·5°K at 100 mb and the corresponding changes at the other els would satisfy characteristic (*b*) (ii) above. The reverse process would have to e place in the afternoon.

In the absence however of direct evidence for these rather complex dynamical cesses it seems more likely that a large part of the observed midday-midnight nges is due to radiation error of the thermometer. Without further experi- ntal evidence it is impossible to be definite.

The daylight ascents at 06 and 18 hr in 1947 take place when the sun is at roximately the same elevation and one might expect radiation errors to be similar. ilarly for the 09 and 15 hours ascents in 1948. If one assumes that the differen- heating only is real, *i.e.* that T_{18-06}, T_{15-09} values are without error, then the tively small daylight rise in temperature shown by these differences 3(*b*) (v) is te consistent in order of magnitude with the nocturnal cooling. They indicate increase of temperature throughout daylight of the order of 0·5°K with the ximum of temperature probably in the afternoon, followed by a similar fall in perature throughout the night.

4. Conclusions

(a) The evidence of the higher reaches of the British radiosonde ascents at ht points to a nocturnal cooling of the 150 — 80 mb layer of the atmosphere of order of 0.05°C hr^{-1}.

The times of ascent now used are particularly valuable, in that the 21 hr and hr flights allow this figure to be determined irrespective of any radiation error he thermometer. They occur in darkness throughout the year, except at Lerwick und mid-summer. With a continued analysis of these flights it should be possible, ime, to gain further precision in the value to be given to nocturnal cooling.

(*b*) It is not possible from the analysis to decide how much of the measured light temperature increases at these levels is due to radiation error.* If one umes that the major part of the apparent diurnal heating, midnight-noon, is false, n flights which take place symmetrically about noon, when radiation errors uld be similar, suggest a rate of heating in the daytime of the same order as the turnal cooling with the highest temperatures in the afternoon.

(*c*) If the measured daylight temperatures are real, then vital difficulties in way of explaining the measured daylight temperature increases as radiative are :

* The experiments of Brasefield (1948) are interesting in this connection in showing what vast differences may be made e apparent diurnal range in the stratosphere by modification of the temperature element and radiation shield.

(i) The magnitude of the heating, midnight-noon, would imply a much larger absorption of solar radiation by these layers than is likely, judging by our present knowledge of the composition of the lower stratosphere.

(ii) The close symmetry about noon of the apparent daylight heating would imply a high rate of emission of radiation which is not compatible with the large heating.

The alternative is to assume that a daytime *dynamic* heating of about 3°K is possible at these levels. Difficulties which then remain are to explain the suddenness of the phenomenon, with the greatest rate of change very near sunrise and sunset and apparently relative quiescence through the night, and also to justify the rather complex air-flow necessary to restrict the diurnal *ground-level* pressure changes to the low values of these latitudes.

If a dynamic process were responsible for the observed changes then conclusion (*a*) would of course no longer give a figure for *radiative* cooling alone.

(*d*) An experimental investigation of the magnitude of thermometer errors in daylight is essential if full value is to be realised from the highly organised programme of ascents in this country. The measurement of the velocity of sound over a short path in the free air at these levels has been attempted (Kay 1949), using a free-balloon ascent, and cannot be achieved at this time with sufficient accuracy ($\pm$ 0·5°C in temperature $\simeq \pm 1 \times 10^{-3}$ in velocity). Perhaps 'scaling' experiments would lead to some estimate, if a sufficient number of sondes could be set aside, fitted with radiation shields and thermometer elements suitably scaled up and down both geometrically and in thermal properties.

(*e*) Until this difficulty has been resolved it is probably unsafe to regard as real the large daytime increases in temperature measured in the lower stratosphere.

Acknowledgments

My thanks are due to my wife for assistance with the calculating machine; to Professor Lord Cherwell for the facilities of the Clarendon Laboratory; to Professor G. M. B. Dobson and Sir Charles Normand for their interest in this work; to Dr. Goldie for stimulating discussions about the possibility of dynamic processes; and to Sir Nelson Johnson and members of the Meteorological Research Committee for making available internal reports of the Meteorological Office.

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Mean upper contour patterns of the northern hemisphere — the thermal-synoptic view-point

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(Manuscript received 13 April 1951)

SUMMARY

The mean 500 mb contour patterns and 1,000-500 mb thickness patterns for the northern hemisphere in January and July are examined. The orographic-dynamical hypothesis advocated by Bolin (1950) is considered. A thermal-synoptic explanation, resting mainly on the direct thermal effects of land and sea modified by baroclinic synoptic processes, is regarded as satisfactory.

1. INTRODUCTION

In recent years a considerable literature has appeared supporting the view that over periods of interest in weather forecasting – a day or two at least – a useful approximation to the evolution of the synoptic flow pattern of middle and upper layers of the troposphere may be inferred from the relatively simple dynamics of inertia flow analogous to that of a barotropic fluid. The ideas derive largely from Rossby's studies (*e.g.*, 1940) and are perhaps most completely stated by Charney and Eliassen (1949). The relevance of the results to the atmosphere is however not based on rigorous theoretical argument ; the point of view has rather the status of a working hypothesis the greatest virtue of which is its mathematical simplicity and the validity of which must be decided by appeal to observations.

It is probably true to say that the hypothesis seems unrealistic to the majority of dynamical and synoptic meteorologists who think of the changes in circulation as essentially three-dimensional in a thermodynamically active baroclinic medium. They would not reject the assumption of quasi-barotropy to be a tolerable first approximation but regard the line of attack as valuable in affording experience in quantitative methods which may in due course be extended to a tolerably realistic formulation of the atmospheric problem.

Be that as it may, it is possible that the claims put forward for the ideas, as applicable to the real problem, may mislead for a time and, until the issue is settled, it is worth while to attempt to maintain the balance by stressing the importance of thermal developmental processes. Bleeker's paper (1950) is valuable here.

Recently the same basic attitude has been introduced into the general circulation problem and Bolin (1950) has ably argued that the main troughs and ridges which appear in the mean contour charts for the middle and upper troposphere are not the net result of complex thermal and synoptic processes but are set by wave disturbances in the westerlies produced by the mountain barriers – especially the Rockies. In support of this thesis the major part of the paper can by a careful, although justifiable, selection of latitudes point to an antiphase relation between the summer and winter trough-ridge patterns when land-sea thermal differences might more naturally suggest antiphase. Also the secondary trough over Europe, for which no thermal explanation is obviously to hand, may be itself a consequence of the wave pattern set primarily by the Rockies.

It is a further inference from the hypothesis that much of the longitudinal variation in mean tropospheric temperature, as evidenced by the warm ridges and cold troughs

broadly in phase with the mean flow patterns, is also determined by the 'orographic-dynamical' control. The basically zonal thermal distribution is advectively distorted in phase with the dynamically controlled mean flow so that the warmth of the east Atlantic is of orographic-dynamical rather than thermal-synoptic origin.

The arguments presented are strong and the thesis is in many ways attractive but it is believed that the following analysis of the same evidence as was used by Bolin will at least show that the more classical ideas need not be abandoned. The charts to be discussed are the mean 500 mb contours for January and July published by Scherhag (1948) and as used by Bolin, and the 1000-500 mb thermal or thickness patterns also given by Scherhag.

2. The thermal-synoptic view-point

An explanation of mean 500 mb contour patterns on thermal-synoptic ideas might proceed as follows. For general synoptic-dynamical reasons, which tend to keep low-level winds generally much lighter than high-level winds, it is necessary that the trough-ridge pattern of the mean upper contours should be broadly in phase with the thermal pattern. This is a consequence of the application of the hydrostatic equation upwards from a boundary with relatively weak pressure differences. We need not inquire here into the fundamental reason for this fact since, presumably, it is not itself a mere consequence of orography. Thus the parallelism between mean isotherms and mean upper contours is not an argument for or against any particular explanation of the patterns. While the orographic-dynamical argument seeks to establish the mean upper flow pattern in its own right and to carry the thermal pattern automatically, the thermal-synoptic argument will seek to show that the thermal field is set by direct thermal processes modified by synoptic baroclinic developments and that the mean upper contour pattern is automatically accounted for. If this argument can stand without appeal to long waves of orographic origin there must at least be scepticism of the orographic-dynamical argument.

3. The January mean patterns

The two charts of relative topography and contours of the 500 mb surface are shown in Fig. 1. The close correspondence between the positions of the ridges and troughs in the thermal and contour fields needs no emphasizing. Referring then to the thermal patterns, we find two dominant troughs which lie, as faithfully as any thermal enthusiast could hope, near the eastern extremities of the two main cold land masses. Climatologically this is entirely acceptable ; synoptic experience would suggest it as almost inevitable for cold air which forms or is maintained freely over the continent is rapidly modified on moving over the warmer sea, a fact regularly used in synoptic forecasting.

Additional to this direct thermal tendency there is the synoptic fact that the western oceanic areas are strongly cyclogenetic. This fact is variously interpreted on frontal theory (*e.g.*, Bergeron 1930), as a direct result of the heating (*e.g.*, Bleeker 1950), or, as we prefer, as a dynamical consequence of the direct thermal trough in accordance with our general theory of baroclinic development (Sutcliffe 1947, Sutcliffe and Forsdyke 1950). These baroclinic developments are unstable – or self-developing – processes which tend further to distort the thermal field so that we find the mean thermal troughs as very pronounced features.

There are also just two marked mean thermal ridges which again lie very faithfully over the eastern oceans in thermally suitable positions. Admittedly, that over the Pacific extends over Alaska and so over part of the Rockies but the main run further south is definitely over the sea and the Rockies mark merely an inflexion similar to that over western Europe. To argue that the Pacific ridge is a Rockies ridge and the Atlantic ridge a mere down-stream dynamical effect seems difficult on the evidence.

The only apparent remaining anomaly is the third trough extending from North Siberia, SW. then S. to the eastern Mediterranean. But, it is urged, an appeal to long

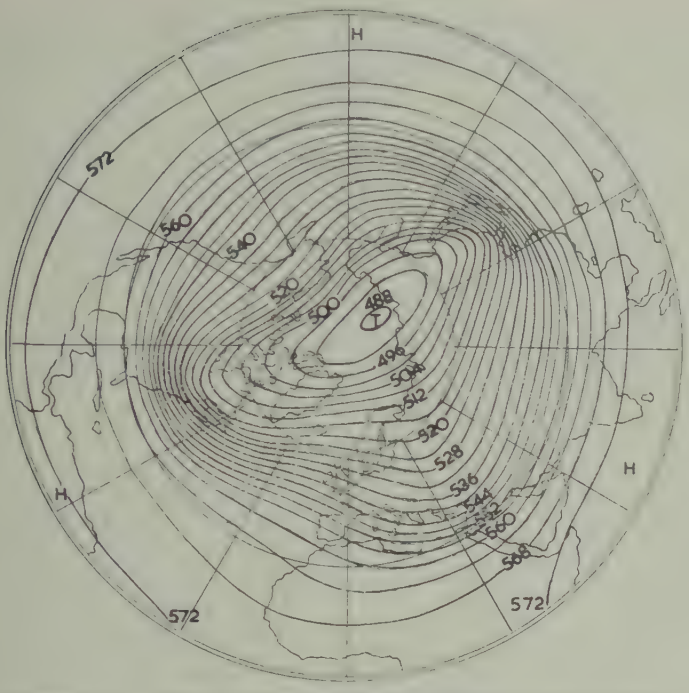

Figure 1 (a). Mean absolute topography (contour pattern) 500 mb, January (after Scherhag 1949).

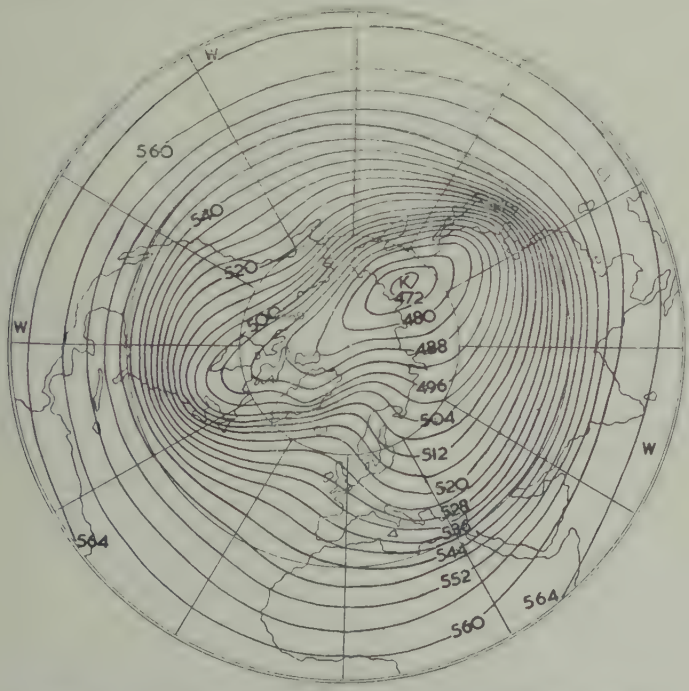

Figure 1 (b). Mean relative topography (thickness pattern) 1,000-500 mb, January (after Scherhag 1949).

Figure 2 (a). Mean absolute topography (contour pattern) 500 mb, July (after Scherhag 1949).

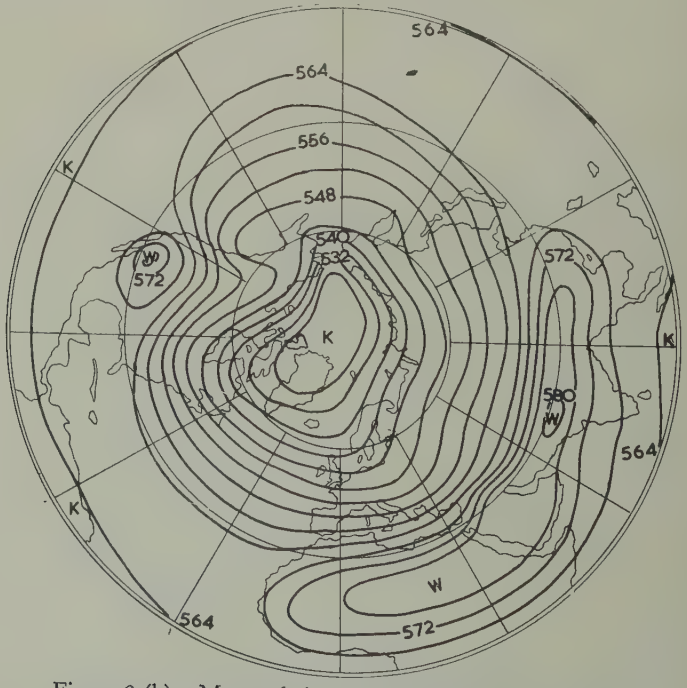

Figure 2 (b). Mean relative topography (thickness pattern) 1,000-500 mb, July (after Scherhag 1949).

ographic wave theory to account for this distortion of the mean isotherms is unnecessary, dinary baroclinic synoptic processes may equally well suffice. As the mean sea level bars indicate, there is a strong mean advection from SW. across the mean isotherms of W. Europe associated largely with depressions moving further to the NW. whereas, er SE. Europe, there is a mean surface easterly drift and cold air advection to the east editerranean. Although, naturally, no complete explanation of the statistical mean ults of widely varying synoptic situations can be obtained from mean charts themselves geographical alignment of this third trough fits well enough with the distortion of the rmal field on the scale of ordinary synoptic features. It is interesting also to note that the thermal pattern, the same tendency to create a secondary trough sharply down-eam of the Pacific ridge can be clearly detected and that it is associated with easterly face drift in the south. This hint of a similar process in North America rather suggests t the insertion of an extra mean trough over Europe is a net result of synoptic develop-nts rather than of orographically controlled upper wave patterns.

4. The July mean patterns

The charts are shown in Fig. 2 and certainly the thermal-synoptic explanation of the an thermal and contour patterns does not fall so readily into the hands. If it did there uld hardly be any excuse for an alternative theory ; the similarity between the January d July patterns is a corner stone in the orographic-dynamical theory. We must of course nt the remarkable coincidence of the mean trough of North America, but looking ewhere is the correspondence so close ? The summer 500 mb ridge which may appear correspond with the Pacific-Alaskan ridge of winter is in fact displaced by some 25° longitude all along its length and lies definitely over the warm continent. There is tead a quite definite trough over the east Pacific and the two seasonal patterns are rather re nearly in antiphase than in phase : here, in the main origin of the hypothetical ographic-dynamical patterns, there seems to be a quite poor similarity between the two sons.

If then we wish to consider a basic thermal explanation we may profitably look at the an thickness lines surrounding the pole and follow say the 5,400 m line. It is quite narkable how this follows the boundaries of the Polar Seas, even in some detail, responding he Bering Straits, the cold Hudson Bay, even the North Sea and the Baltic. The coldness the waters of Canada is a climatic fact of much significance and greatly reduces the rate modification of northerly wind currents in summer ; as Burbidge (1951) remarks, tario and Quebec have an Arctic Ocean as their northern boundary. Now it cannot rly be argued that these seas are cold in summer because the winds induced orographically from the NW. ; they are cold as a persistence from winter ice. Rather may we argue t the atmospheric cold trough is maintained from the cold surface and emphasized by the lonic polar outbursts developing as a baroclinic consequence. A thermal-synoptic planation of this summer trough is therefore not difficult. Similarly if we start with European trough as thermally controlled by the coolness of the North Sea, the Baltic, Mediterranean and the Black Sea we have at once a fair explanation of the mean 500 mb ntour trough and incidentally of the frequent post-trough anticyclogenesis of western rope (shown by the extension of the Azores high in the mean isobars) and of the cyclonic vity of the Middle East. Further east there is a rapid heating giving a thermal ridge at ery short half wave length (compare the very short half wave length of east America) owed by weaker features which it is hardly profitable to discuss on mean patterns alone.

By selection of latitudes Bolin suggests a correspondence between the winter trough, ich extends from North Siberia to the Mediterranean, and the summer trough which een to extend from Iceland to the Mediterranean. Of course orographic-dynamical ories would not predict similar wave lengths in the two seasons but the differences in ntation in these features does not suggest a similar origin.

On the whole the claim for a direct thermal explanation of the mean thermal patterns so of the mean upper contour patterns seems quite strong in summer as in winter.

5. Conclusions

Mountain masses are certainly important as climatic divides, as has long been recognized. There is no doubt that the presence of a western wall to North America and of a southern wall to Eurasia is climatologically of great significance. The obvious and direct effect is however to prevent the free advection of lower tropospheric air across the barrier and so to introduce sharp differences of thermal and moisture conditions. It is also well recognized that mountains are largely responsible for many features of the synoptic chart, particularly lee depressions, and it is to be expected that they will have some effect on mean upper flow patterns in any theory.

There is however little convincing synoptic evidence to suggest that orographically induced long waves of a quasi-barotropic nature have much significance in the atmosphere since baroclinic developments are invariably present to confuse the issue. Moreover with non-linear differential equations as the basis of theory, a proof that orographic long waves may exist in some idealized atmosphere (see Queney 1948) does not imply their probable existence in a real atmosphere always subject to baroclinic developing disturbances of a different nature. There is then no strong *a priori* reason for expecting such waves to dominate on statistical mean patterns. Furthermore, while Charney & Eliassen (1949) introduce orographic effects into their analysis they find them unimportant for practical computing.

The analysis of the 500 mb mean absolute and relative contour patterns (flow patterns and thickness patterns) for January and July shows that the orographic-dynamical explanation runs into difficulties and that even in the vicinity of the Rocky Mountains the summer and winter patterns are more nearly in antiphase than in phase.

It is concluded that the main features of the mean tropospheric temperature distribution are explicable on direct thermal grounds and that the mean upper contours are largely controlled thereby. Baroclinic development of depressions and anticyclones is statistically biased by this direct thermal distribution and introduces modifications notably in intensifying by advection the basic thermal troughs and ridges. The observed mean contour patterns are satisfactorily in accord with these thermal-synoptic principles.

Acknowledgment

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The size of cloud particles in stratocumulus cloud

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Summary

The size distribution of water droplets in thin stratocumulus cloud has been investigated using impactor technique. The variation from point to point in the cloud is considered and it is shown t where the water content is large there is generally a larger number of particles but the average e is also greater. It is remarked that it seems to be quite usual for this sort of cloud to have a double ximum in the size distribution of droplets.

1. Introduction

Diem (1942 and 1948) has described a technique for measuring the sizes of cloud rticles in natural clouds. In this technique all the droplets in a very small sample of e cloud are measured and counted ; in most other techniques (see, *e.g.*, Jones and Lewis 49) a large volume of cloud is sampled and the information obtained, generally rather igh, relates to an average over that volume. We have used Diem's technique to invest- ate the variation of droplet sizes and size distribution from point to point within con- uous sheets of stratocumulus clouds.

2. Apparatus

Fig. 1 represents a cross section of the apparatus. *EF* is a microscope slide coated th magnesium oxide. There is a shutter (not shown) arranged to expose each part of e slide for 0·01 sec. During this time a volume of some 250 cm^3 of air is sampled.

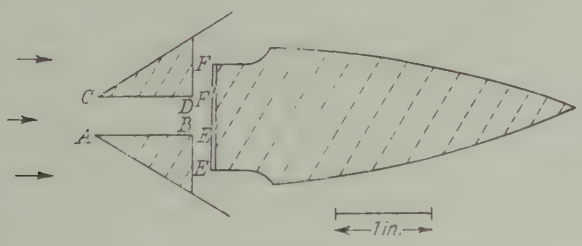


Figure 1. Cloud-droplet impactor.

Droplets sufficiently large to be practically undeviated as the air turns through *BE'* d *DF'* will strike the slide between *E'* and *F'*. Smaller drops will be more or less deviated t all drops greater than some critical size will strike the slide somewhere between *E* and *F*. e have, by analysis of a large number of slides, obtained a picture of the spectrum of op sizes across the slide ; and from this picture we can obtain a rough idea of the percent- e catch of droplets smaller than the critical size.

It appears that droplets larger than 10 μ diameter are undeviated and strike the plate tween *E'* and *F'* ; the critical size is about 4·5 μ, *i.e.*, there is no significant loss of particles diameter greater than 4·5 μ.

May (1945) gives figures for estimating the impactor efficiency of this type of apparatus. s seen from his graphs that if there is just 100 per cent efficiency for particles of diameter

4·5 μ then the efficiencies for diameters 3·5 μ, 2·5 μ and 1·5 μ are approximately 90, 50 and 10 per cent. Those figures fit in quite well with the spectrum picture obtained by direct counting, and all our results have been adjusted on this basis. The adjustments make very little difference to the total amount of water but make an appreciable difference to the total number of droplets.

May, in an unpublished *Ministry of Supply Report*, has investigated the relationship between the droplet size and the resultant crater in the *Mg*0 coating. He finds that, for droplets of diameter greater than 20 μ, and for large impingement velocities, the droplet size is about 0·85 of the crater size. There is some uncertainty as to the factor for smaller drops but all available evidence suggests that the same figure of 0·85 is appropriate. All the measurements have therefore been adjusted on this basis.

3. Accuracy

There are two sources of error : (a) incorrect deductions about the size of the droplets from the size of the craters ; (b) selective sampling.

Of these, (a) was discussed above and it is unlikely that errors on this account will be large, nor will they materially affect comparison between samples taken at different places in the same cloud.

Wind-tunnel tests have shown that there is very little turbulence in the throat of the impactor, and that the air speed in the throat is very little different from the speed in the undisturbed air stream. The only serious errors due to selective sampling will therefore accrue from the variable efficiency of catch discussed in Section 2. For droplets larger than 10 μ these errors are almost certainly very small. For smaller drops the errors increase but, again, these errors should not affect comparisons between samples taken at different points in the same cloud.

4. The scale of the variations

It has been stated earlier that the main advantage of this method of investigating the sizes of water droplets in a cloud is that a detailed examination is made of a small volume of cloud. The volume sampled with each exposure is about 250 cm^3, being the volume swept through by the exposure area (2·5 cm^2) in 0·01 sec (1 m). In practice a sample count is taken over a narrow strip across the slide so that the droplets counted and measured are those which were in the volume swept through by an area of about 0·02 cm^2, *i.e.*, a volume of about 2 cm^3. Repeated tests show that such samples are always quite representative of the whole volume of 250 cm^3.

The sample is representative of an even larger volume. Thus, two exposures made in rapid succession (100 to 200 m apart) generally, though not invariably, gave similar results. Exposures made 3,000 m apart are generally quite different. It seems therefore that each sample is representative over horizontal distances of the order of 100 m.

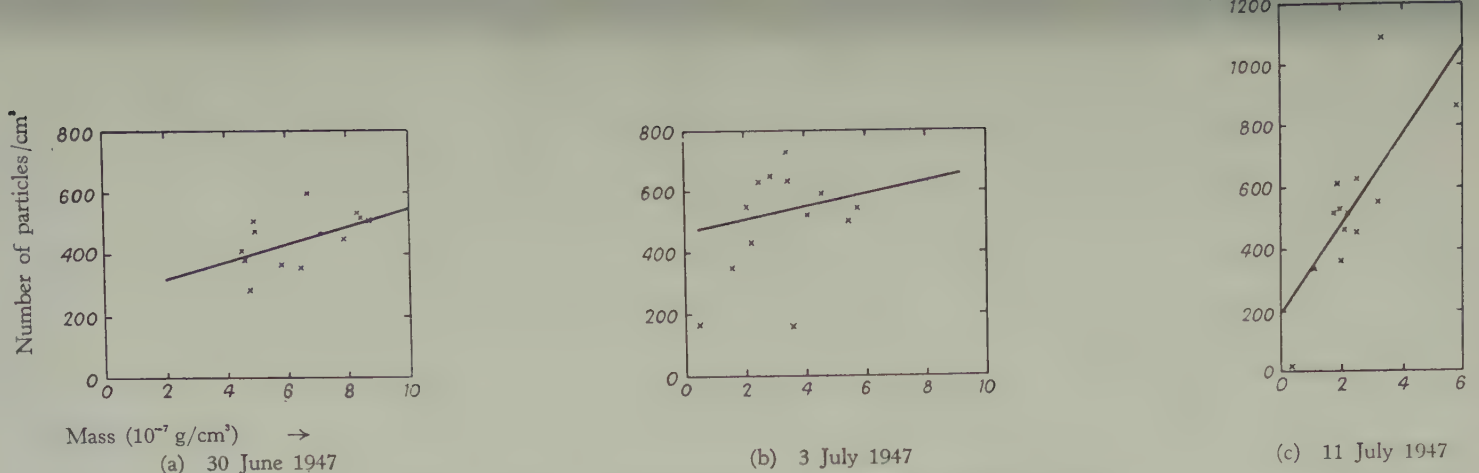
5. Results

(a) *Presentation*

Each slide, after exposure, was examined under a microscope and the craters compared is size with a series of circles, whose diameters increase in multiples of 2. For convenience this series of circles is numbered 1, 2, 3 *etc.*

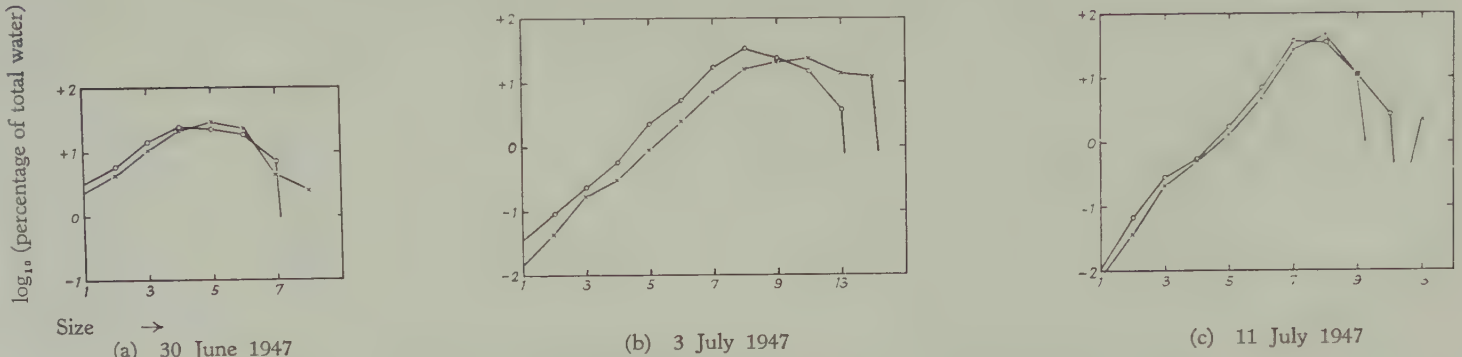
It is often desirable to present results of such counts graphically in the form of frequency curves. Custom seems to require that the abscissa of such frequency curves shall be the diameter of the droplets. We cannot see the justification for this. Indeed since for most purposes the real interest lies in the volume of water present, it would seem more reasonable to use the volume of the droplets as the abscissa.

In practice, however, it is more convenient to use the ' size ' (1, 2, 3 *etc.*). This may be regarded as an abscissa of either log diameter or log volume, or, for that matter, log surface-area.



(a) 30 June 1947 (b) 3 July 1947 (c) 11 July 1947

Figure 2. Water content and number of droplets in individual samples.



(a) 30 June 1947 (b) 3 July 1947 (c) 11 July 1947

o——o Below-average water content x——x Above-average water content

Figure 3. Mean size-distribution curves. Abscissae (sizes) are defined in Table I. The scale for (a) differs from those for (b) and (c).

(b) *The variation of water content and number of droplets*

Three flights have been made. Each was in shallow stratocumulus (1,000 to 2,000 ft deep) and a series of about 12 exposures made in each flight over a distance of some 15 mi (5 min flying). Fig. 2 shows N (the number of droplets per cm^3) plotted against W (the water content in g/cm^3) for the three flights. There is considerable scatter but it is clear that at a fixed level in any one cloud there is some correlation between W and N, W tending to increase with N. This correlation is indicated by regression lines on W.

(c) *The relation between the water content and the droplet-size distribution*

The total number of droplets present is not known because of sampling errors at the small end of the spectrum of drop sizes. But the amount of water contained in hese small droplets is insignificant ; therefore the total amount of water present is quite accurately known.

We have divided the samples from each set into two groups, in one of which the water content is above average and in the other below average. The percentage distribution of water amongst the drops of various sizes was computed for each sample, and a mean percentage distribution found for each group. These are shown in Fig. 3 and the curves agree in indicating that when the water content is higher than average the extra water is mostly contained in drops of the larger sizes. This is, of course, obvious from the slope of the regression lines in Fig. 2.

TABLE I. Diameters of drops of given 'sizes'

					30 June 1947							
Size	1	2	3	4	5	6	7	8				
Range of diameters (μ)	5·1 – 7·2	7·2 – 10·0	10·0 – 13·8	14 – 20	20 – 27	27 – 39	39 – 55	55 – 78				
					3 and 11 July 1947							
Size	1	2	3	4	5	6	7	8	9	10	11	12
Range of diameters (μ)	1 – 1·5	1·5 – 2·1	2·1 – 3·0	3·0 – 4·2	4·2 – 6·0	6·0 – 8·5	8·5 – 12·0	12 – 17	17 – 24	24 – 34	34 – 48	48 – 68

(d) *Double maximum*

It is interesting to note that the 'above-average' water-content curve for 3 July, and both curves for 11 July, show clearly a secondary maximum at size 3 (2 to 3 μ). At the time of the first run we did not have facilities for high magnification, so that droplets smaller than 5μ could not be measured ; however the curves for 30 June also seem to show the beginning of a secondary maximum.

This double maximum has also been found in sheets of Ac and it seems likely that it is a common feature of thin sheet cloud.

Acknowledgment

I have to thank the Director, Meteorological Office, for permission to publish this note.

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551.508.765
551.578.11

A raindrop spectrograph

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(Communicated by C. H. B. Priestley ; manuscript received 30 January 1951)

Summary

A simple form of raindrop spectrograph is described. It is capable of giving a continuous record f the changes in the distribution of drop sizes which occur during a fall of rain.

Examples are given of the spectra obtained during a variety of rain situations ranging from con-nuous rain to isolated showers.

1. Introduction

The best known method of measuring the distribution of drop sizes in rain is that sed by Lenard (1904) and Defant (1905), in which the raindrops are allowed to fall on lter paper sensitized with a dye such as rhodamine. Each raindrop forms a stain, the iameter of which is related in a simple way to the size of the raindrops which produced it.

In another method the raindrops fall into finely divided flour, in which they form eads of dough. After treatment, the size of the beads can be related to the size of the riginal drops. The method is not an attractive one at first sight, but it was used success-ully by Bentley (1904), and in the hands of Laws and Parsons (1943) it gave excellent uantitative results.

Both methods require a considerable amount of labour in analyzing the results and are nconvenient when the drop-size distribution is changing rapidly.

A third method developed recently is one in which the raindrops are allowed to impinge n a microphone and their mass, and hence their diameter, is determined from the magnitude f the impact. The method is useful for remote indication, and has been developed by Cooper (1951) in the form of a balloon-borne instrument for measuring the spectrum of aindrops at different heights above the ground. The method is capable of giving a direct ndication of the form of the spectrum, but has the disadvantage of requiring complicated lectronic equipment.

An important requirement for investigations of the physics of rain formation is a simple nstrument which is capable of giving a continuous record of the raindrop spectrum. The present paper describes a device of this kind, and gives an indication of the results btained with different types of rain.

In principle, the device is a form of mass spectrograph in which falling raindrops re deflected by a horizontal air current. This is accomplished by allowing a sample of the rops being investigated to fall through a small orifice in the top of a low-velocity wind unnel. The drops are deflected downstream a distance which is approximately inversely roportional to their mass, so that if the sample contains a distribution of drop sizes, they re spread out along the bottom of the tunnel according to size. There they impinge on ensitized filter paper, and if this is moved slowly at right angles to the air stream, a con-inuous record is obtained showing the variations in the spectrum with time.

2. Construction

A spectrograph has been constructed along these lines with a tunnel approximately ft long having a rectangular working section 4 in. wide and 10 in. deep, as shown in Fig. 1. Honeycombs are placed at both ends to obtain a uniform air flow, and an air velocity of 8 ft/sec is maintained by means of a small electric fan. Near the centre of the tunnel t 0 is a circular aperture 1·5 in. in diameter, surrounded by a funnel F through which the aindrops fall. The orifice in the funnel is also circular and is accurately 1 in.^{2} in area.

Since the deflection of the drops is inversely proportional to D^3, where D is the diameter f the drops, the deflection of small drops is excessive compared with that of the larger ones.

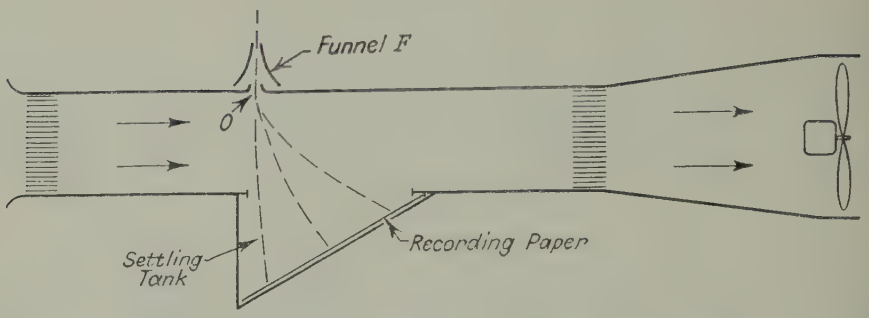


Figure 1. Schematic diagram of the raindrop spectrograph.

The deflection of the larger drops may be increased by adding a settling tank in the form of a wedge as shown in Fig. 1. On entering the relatively still air in the tank the larger drops continue with their previous trajectory, and hence have an increased deflection compared with the smaller ones. This device does not seriously affect the flow of air in the main part of the tunnel and as a result the deflection of the drops is more nearly proportional to $D^{-1.5}$ than D^{-3}.

An important point of detail in the construction of the tunnel is the design of the funnel F. Without it, drops falling on the top of the tunnel cause splashes to enter the orifice and appear on the record as small raindrops. Secondly, drops which hit the edge of the orifice break up and produce a similar result. The present design has been found by experience to reduce both effects to negligible proportions. Splashes are prevented from entering, while drops which hit the edge of the funnel run down the inside surface and are prevented from falling into the tunnel by the subsidiary lip at 0.

The recording paper used is No. 1 Industrial filter paper, lightly dusted with rhodamine dye, and prepared in lengths of 6 to 8 ft, approximately 1 ft wide ; it is fed through the tunnel on rollers driven by an electric motor. A speed of 1 in./min is adequate for light rain, but a speed of 3 in./min is required for moderate to heavy rain.

As it is important that drops should be travelling in a vertical direction on entering the spectrograph, the instrument is installed near the centre of a courtyard 112 ft by 34 ft surrounded by walls 40 ft high. It is also desirable to place a funnel a few feet in diameter around the orifice to protect the drops from draughts in the last part of their fall.

These precautions are usually sufficient to ensure that raindrops fall in a nearly vertical direction into the tunnel, but the instrument must nevertheless be used with discretion, and the results rejected if the wind conditions are such that the drops are not falling vertically in the vicinity of the tunnel.

3. Calibration

Owing to the complication introduced by the settling tank, no attempt has been made to calculate the trajectories of the drops and these have been determined by calibration. Drops of accurately known size are allowed to fall into the tunnel, their deflection is measured and a calibration curve drawn. The curve produced in this way for an air speed of 18 ft/sec is given in Fig. 2.

In deriving a curve of the distribution of drop sizes, or in calculating rainfall intensity from the records, a correction must be applied for edge effects at the orifice through which the raindrops fall. If the diameter of the orifice is a, its effective area for drops of diameter D is $\pi (a - 2D)^2/4$. Numerically the correction factor is 7 per cent for drops 1 mm in diameter and is proportionally smaller for smaller drops.

The range of drop sizes which may be resolved in an instrument of the dimensions given is approximately 100 to 1 in mass, or 5 to 1 in diameter. As the full range of raindrop diameters is nearly 25 to 1, either a larger instrument is required, or one with two ranges to cover the whole spectrum at once. The instrument is not suitable for drops smaller than 0.25 mm in diameter as these leave a poor trace on the recording paper.

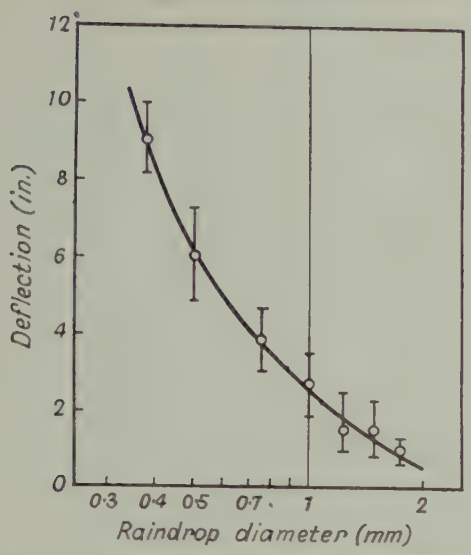


Figure 2. Calibration curve of the spectrograph for airspeed of 18 ft/sec.

The present design is suitable for drop sizes from 0·3 mm to 1·5 mm in diameter ıd is therefore useful for light to moderate rain. Recordings of the total rainfall rate ıve been made simultaneously with the spectrograph observations and, subject to the nitations mentioned above, the results compare well with those obtained by integration the drop-size distribution curves.

4. Typical results

Some typical results obtained with the instrument are given in Figs. 3 to 6. These ıve been prepared by photographing tracings made direct from the original records, the dinates and abscissae being respectively raindrop diameter and time. They have been osen to illustrate the principal types of spectra which have been obtained with continuous in and isolated showers, respectively.

(a) *Spectra from continuous rain*

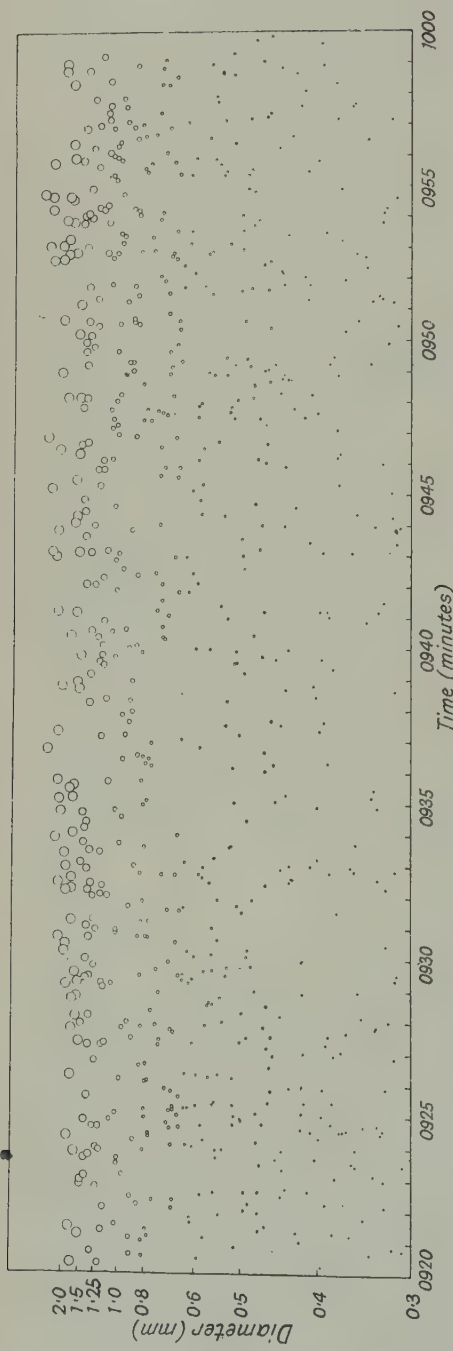
Fig. 3 is a record of 40 min duration obtained during a longer period of continuous in. It shows practically no change in the spectrum with time.

Fig. 4 shows a different type of record, which is obtained rather more frequently an the previous one in SE. Australia. There are frequent, marked changes in the spectrum, owing that a spectrum determination at one particular time would be quite unrepresenta- e of the average over the period.

(b) *Spectra from showers*

A characteristic pattern obtained in showery conditions is given in Fig. 5. This rticular shower lasted approximately 20 min. The greater part of the fall took place er the observing point and the shower did not move across the station to any extent. ıere is a distinct tendency for the larger drops to appear first, followed by the smaller ops some minutes later. This may be interpreted as due to the difference in rate of fall the smaller and larger drops.

Fig. 6 shows the spectra of two showers, each of approximately 5 min duration, separated an interval of 5 min. All the drops tend to arrive at approximately the same time, espective of size ; this is presumably due to columns of rain passing over the recording tion, each record representing a sample of the spectrum at the relevant time.



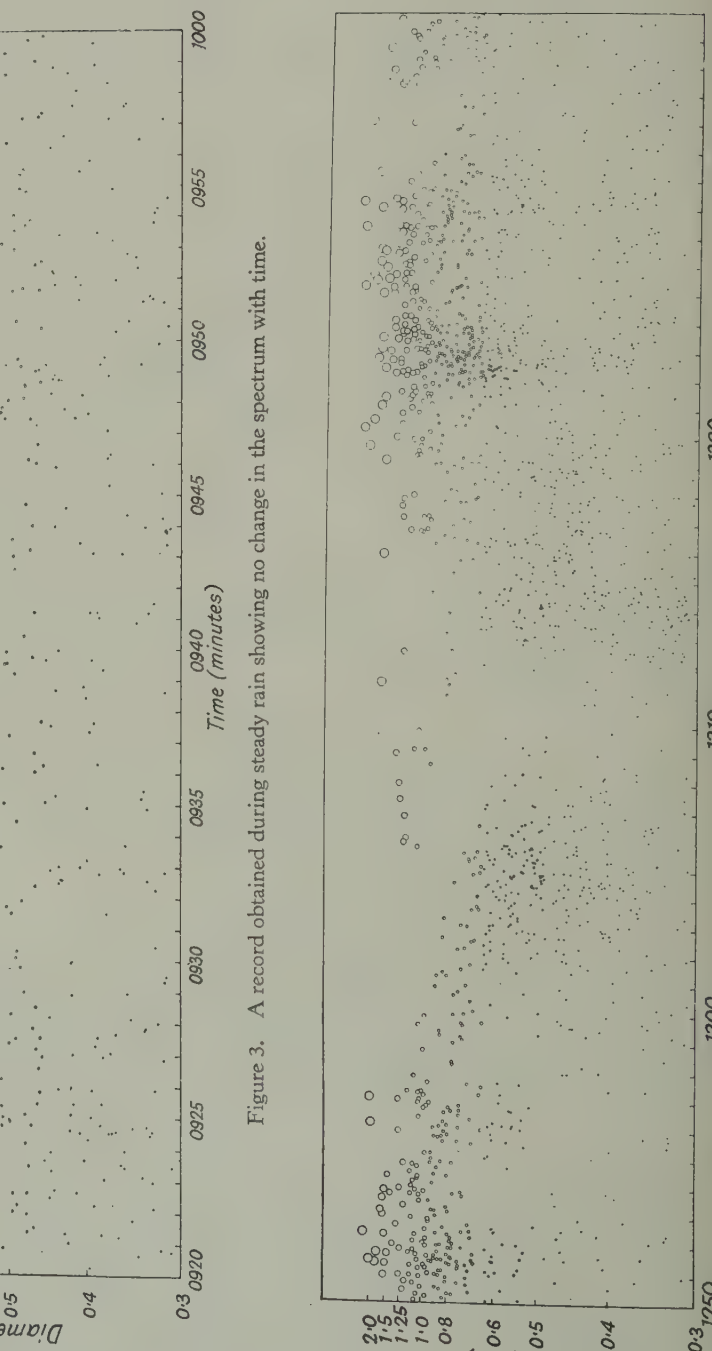


Figure 3. A record obtained during steady rain showing no change in the spectrum with time.

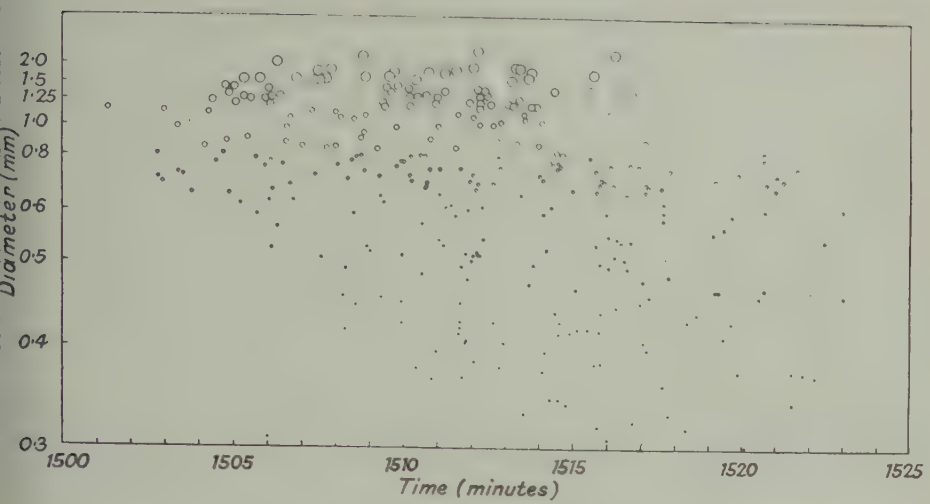


Figure 5. A typical record of a shower.

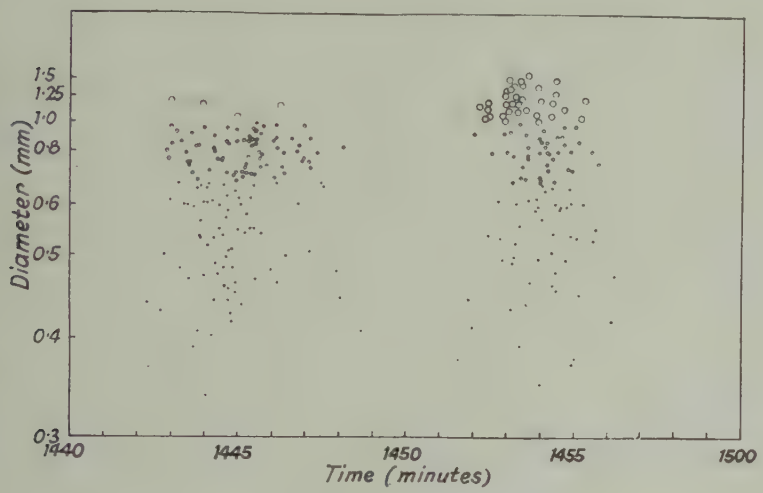


Figure 6. A record of two short showers.

5. Conclusions

The raindrop spectrometer is intended for use in studies of the physics of natural
n formation. It is likely to prove of greatest value when used in association with radar
uipment which is capable of giving information about the mode of origin of the raindrops,
ir growth and their motion before falling in the general vicinity of the spectrograph.

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551.508.71

On the direct measurement of humidity mixing ratio and its fluctuations

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(Manuscript received 24 January 1951)

SUMMARY

An apparatus is described for the direct measurement of humidity mixing ratio. It is designed primarily for recording the turbulent fluctuations in the mixing ratio in the lower atmosphere, and is based on an electrical analogue of the wet-bulb equation, using dry- and wet-bulb thermocouples.

Field tests give agreement with Assmann psychrometer values to within less than 1 per cent under favourable conditions for comparison.

1. INTRODUCTION AND THEORY

The study of the turbulent transfer of water vapour in the atmosphere involves the study of the fluctuations in the humidity mixing ratio but heretofore no method has existed for measuring the latter quantity directly ; it has been necessary to measure at least two other quantities and compound them. Such methods are too laborious for application in turbulence studies and we have therefore developed a direct indicator of the mixing ratio. This is given by the deflection of a galvanometer to the terminals of which are applied appropriate fractions of the e.m.f.'s from dry- and wet-bulb thermocouples as required by the wet-bulb equation.

In more detail, the humidity mixing ratio r is given by the familiar wet-bulb equation (in its sufficiently accurate form)*

$$r = r_w - \frac{c_p}{L}(T - T_w) \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where r_w is the saturation mixing ratio at the wet-bulb temperature T_w, T is the dry-bulb temperature, c_p the specific heat of air and L the latent heat of vapourisation at T_w. Now r_w at a given pressure – we are only concerned with observations taken near mean sea level – is a function of T_w only and may be represented, over a *restricted* range of temperature centred on a datum T_{0w}, by a linear relation (*cf.* Section 2(a)),

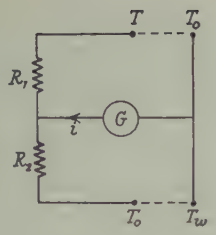
$$r_w = r_{0w} + m\,(T_w - T_{0w}) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where r_{0w} and m depend on the chosen T_{0w} and are obtained from saturation-vapour-pressure tables (m increases by about 5 per cent of itself per °C through the range 0°C to 25°C.). Substituting Eq. (2) in Eq. (1)

$$r = (r_{0w} - mT_{0w}) - \left\{ \frac{c_p}{L} T - \left(m + \frac{c_p}{L} \right) T_w \right\} \quad . \quad . \quad (3)$$

Thus if outputs proportional to $(c_p/L)\,T$ and $(m + c_p/L)\,T_w$ are opposed across the terminals of a galvanometer the resulting deflection from a datum determined by the first bracket on the right of Eq. 3 will be proportional to r.

* Psychrometer tables in common use for fully ventilated wet-bulbs are not usually calculated from this theoretical equation but they agree with it to within about 1 per cent.



gure 1. Thermocouple circuit for obtaining vanometer deflection proportional to humidity xing ratio. Full lines represent one metal pper), dotted lines second metal (constantan).

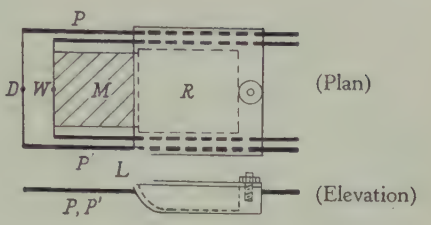


Figure 2. Thermocouple hygrometer (not to scale)
D, *W* = dry, wet junctions, *P*, *P'* = prongs, *M* = muslin, *R* = reservoir, *L* = lid for reservoir.

Using thermocouples, we measure T and T_w with respect to some reference temperature T_0, that is we replace T by $(T - T_0)$ and T_w by $(T_w - T_0)$ in Eq. (3) and so obtain

$$r = a + \left(m + \frac{c_p}{L}\right)(T_w - T_0) - \frac{c_p}{L}(T - T_0) \quad . \quad . \quad . \quad (4)$$

here $a = r_{0w} + m(T_0 - T_{0w})$. The electrical circuit corresponding to Eq. (4) is shown Fig. 1 in which adjustable resistances R_1, R_2 (taken to include the resistances of the ermocouples (T, T_0), (T_w, T_0) respectively) provide the means of applying the appropiate fractions of the thermocouple e.m.f.'s to the galvanometer of resistance G.* If e the thermoelectric e.m.f. per °C for the thermocouples, Kirchoff's laws give for the rrent i through the galvanometer

$$i = \frac{e}{R_1 + G(1 + R_1/R_2)} \left\{ \frac{R_1}{R_2}(T_w - T_0) - (T - T_0) \right\} \quad . \quad (5)$$

mparing this with Eq. (4) we see that i is a measure of $(r - a)$ and therefore of r when

$$R_1/R_2 = 1 + mL/C_p$$

here L, whose variation with temperature is only very slight, is appropriately referred the temperature T_{0w}. Having set R_1/R_2 for the value of T_{0w} about which T_w is ctuating, Eqs. (4) and (5) give

$$r - a = \frac{c_p}{eL} \left\{ R_1 + G(1 + R_1/R_2) \right\} i \quad . \quad . \quad . \quad (6)$$

his equation will then be valid over the range of T_w for which Eq. (2) is a satisfactory proximation to the saturation mixing ratio. The variation of R_1/R_2 with T_{0w} is as follows :

T_{0w} (°C)	4	8	12	16	20
R_1/R_2	1·87	2·11	2·40	2·76	3·18

The ratio R_1/R_2 being determined it remains to choose the actual values of R_1, R_2 as to provide a satisfactory sensitivity with the galvanometer in use, or, of equal importance hen studying fluctuations, to provide suitable damping. Thus to obtain effectively en response to all fluctuations as long as or longer than the free period of the galvanometer, $R_1 + 1/R_2 \simeq 1/R_c$ where R_c is the external resistance for critical damping.

* Since the present work was submitted Swinbank (1951) has published a paper giving an alternative electrical analogue he wet-bulb equation. His method involves three junctions exposed to the air (one dry junction and a pair giving the erence, dry-wet) in place of the present two, and is a more direct analogue of Eq. (1).

2. Scope of method

(a) *Effect of assumption of linearity in r_w, T_w*

The range of practical validity of the method depends on the adequacy of Eq. (2) to express the variation of r_w with T_w. The error arising from the assumed relation is obtained by subtracting the right hand side of Eq. (1) from the right hand side of Eq. (3) and is found to be less than 1 per cent of r for $| T_{0w} - T_w | \leqslant 2°C$ when $5°C \leqslant T_{0w} \leqslant 20°C$ and the relative humidity is greater than about 30 per cent. A fluctuation of ± 2 °C about the mean wet-bulb temperature in a period of 10 min or so is probably unusually large so that Eq. (3) is quite adequate for practical purposes.

(b) *Effect of difference between assumed and true mean wet-bulb temperatures*

The approximate value of T_w is known, by Assmann psychrometer or otherwise, in advance of a run to determine the temporal variation in r. This value of T_w may be taken as T_{0w} in Eq. (3) and the resistance ratio adjusted accordingly. In general however this setting will prove to be more or less in error, as shown by the true mean wet-bulb temperature determined during the run. Any such discrepancy may be allowed for by applying an appropriate scale factor to the record. This factor is the ratio of the true to the indicated value of r, the former being given by Eq. (1) using the true value of T_{0w} for T_w and the latter by Eq. (3) using the assumed value of T_{0w}, not only for T_{0w} itself and T_w, but for the dependent quantitites r_{0w}, m and L also. It is a function of the difference between assumed and true T_{0w} and of the humidity expressed, for example, in terms of T_{0w} and $(T - T_{0w})$, but is constant to within ± 1 per cent provided that the error in setting for T_{0w} does not exceed about 2°C and the fluctuations of relative humidity remain within about ± 20 per cent. It is not difficult to ensure compliance with such requirements.

3. Instrument design

The experimental realization of the instrument represented by Fig. 1 presents no problem as far as thermocouple psychrometry itself is concerned but we may refer briefly to methods adopted to obtain an instrument of rapid response suitable for following atmospheric fluctuations in mixing ratio.

The psychrometer (Fig. 2) consists of a plastic reservoir which supports the dry and wet thermojunctions. The reservoir carries two pairs of stout prongs, about 2 cm apart, of the metals, copper and constantan (or nickel), used for the junctions. Fine wires of these metals are stretched across the ends of the prongs, with junctions (soldered or sport-welded) formed at the mid-points, one behind the other, as shown in Fig. 2. The inner junction forms the wet-bulb and is maintained wet by means of a sheet of very fine muslin led horizontally from the reservoir to the junction, round the latter and back to the reservoir, surface tension keeping the upper and lower sheets of muslin in intimate contact. Variations on this design are to use thin rice paper instead of muslin, or to lay the fine wire junction across the top of the muslin which is then held at its extremity by an additional support. The latter arrangement gives a substantially thinner but nevertheless definite layer of water round the wet-bulb junction.

The time-constants of such thermometer bulbs have been measured in the wind tunnel and found to agree quite closely with theoretical values deduced from heat- and mass-transfer theory (see *e.g.*, Goldstein 1938). There is no difficulty in obtaining a 'fast' dry-bulb ; for example a junction with minimum overlap made from 0·0025 cm diameter (50 s.w.g.) wire has a time constant of about 0·01 sec and one of 0·012 cm diameter (40 s.w.g.) wire, 0·13 sec ; both in a wind of 5 m sec^{-1}, with wind normal to the wire. The mass of water on the wet-bulb adds much to the time-constant of the latter and a value of about 0·2 sec in the above conditions is the best that has been obtained so far. Methods of obtaining thinner films of water on the wet-bulb are being investigated.

In all cases the minimum width of muslin necessary to give full wet-bulb depression was determined by wind-tunnel tests – about 1 cm has been found sufficient for the stoutest wires used.

The choice of galvanometer depends on the time-constants of the thermocouples d the realization of a suitable sensitivity. For the preliminary tests a relatively sluggish vanometer was suitable and a 2Ω, 2 sec instrument has been used. For field work a , 0·4 sec instrument has been mainly used so far but an 0·1 sec instrument is now available en the faster type of wet-bulb is used. Care must of course be taken to use a galvano-ter with time-constant longer than the slower of the junctions if errors in the humidity ctuations due to phase difference are to be avoided. Operating sensitivities have been out 1 cm to 1 g kg^{-1} in mixing ratio.

4. Tests of hygrometer

The instrument was initially tested in the wind tunnel and good agreement having en obtained between the value of r given by an Assmann psychrometer and that from the cuit using Eq. (6), tests were extended to the field. An occasion was chosen on which humidity fluctuations were slight (overcast sky and fresh wind) so as to minimise the ors of the means due to the fluctuations. The thermocouple psychrometer was set ,* facing into wind, at a height of 1·25 m over a grass sports field with an Assmann psychro-ter at the same level. The results from six consecutive 2 min runs, the thermocouple ues being taken from a photographic trace of the galvanometer deflection, were as lows :

	Thermocouple circuit		Assmann
Mean r (g kg^{-1})	8·50 ± 0·09		8·48 ± 0·09
Range	8·32 — 8·62		8·36 — 8·65
Differences, max.		0·14	
mean		0·02	

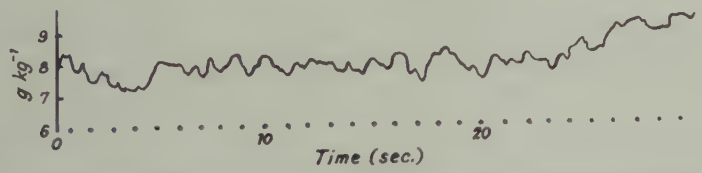
Tolerances are standard deviations.

Observations on two other occasions with bright sunshine and an unsteady value the mixing ratio gave values for the latter of

Thermocouple circuit	Assmann
7·33 ± 0·25 g kg^{-1}	7·23 ± 0·16 g kg^{-1}
7·86 ± 0·24	7·93 ± 0·13

ich are not unreasonable in view of the standard deviations.

An example of a record obtained during a period of fluctuating humidity is shown Fig. 3.



ure 3. Example of record of fluctuating humidity mixing ratio at 1·25 m over short grass surface.

Acknowledgments

We are indebted to Mr. H. Charnock of this department for the suggestion to use electrical analogue and to Mr. Jarman for determining time-constants of the dry- and t-bulb elements.

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* No radiation screen was used as this becomes unnecessary with very small thermometer 'bulbs' formed from fine in winds above about 1 m sec^{-1}. (Work to be published separately).

Correspondence

Sunshine in the British Isles

The accompanying diagram shows the cumulative excess (or deficiency) of bright sunshine as recorded over the British Isles generally, and is similar in construction to that by W. N. McClean to show the rainfall experienced over the Thames Valley from 1880 to 1950 (see *Cent. Proc. R. Met. Soc.*, 1950, p. 227). The two diagrams are interesting because of the differences in the general trend shown by the sunshine and rainfall curves.

The basis of the present diagram is the mean for the period 1909-1948. A rise in the curve for any month signifies therefore an excess, by the amount indicated, over the 1909-1948 average for that month. The greatest rise occurred in 1911 – the sunniest year – and the greatest fall in 1912 – the dullest year, as shown by the figures given in the *Q.J.*, **77**, p. 129.

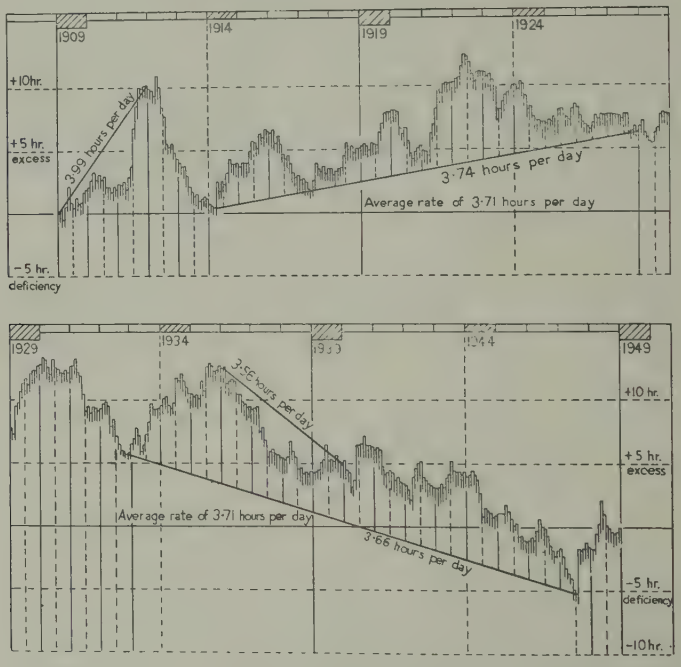

Figure 1. Accumulated excess or deficiency of sunshine over the British Isles, month by month, relative to the monthly mean sunshine for the period 1909-1948. The distance of each ordinate above or below the preceding ordinate is the excess or deficiency of sunshine for that month in relation to the mean for the month.

The curve shows that by the end of 1911 the excess was 10 hr, *i.e.*, the cumulative excess represented 10 hr/day for a month. An excess of 10 hr/day spread over the three years 1909 to 1911, or over 36 months, is equivalent to ·28 hr/day over that period. The mean number of hr/month during this period is therefore 3·71 + ·28 or 3·99 hr/day as indicated on the diagram.

An interesting feature of the diagram is the general increase in the sunshine from 1909 to 1929 and 1935, and the subsequent decline in the general sunshine. These monthly values of sunshine are obtained from the maps published in the *Monthly Weather Report* (Meteorological Office), using 40 points uniformly distributed over the British Isles.

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Large-scale vortex motion in the atmosphere

Mr. R. W. James's paper on the theory of large-scale vortex motion in the atmosphere (*Q.J.*, **76**, p. 255) will interest particularly those meteorologists who consider it to be of some importance to find an analytical means of treating the depression as an entity. My own empirical approaches to this problem a number of years ago, and again more recently (when more adequate upper data became available), were prompted by a feeling that a lot still needed to be learnt about the structure and life history of depressions before much progress could be made on the mathematical side. But a paper such as that by Mr. James stimulates thought and invites criticism.

I wish just to mention two points. Mr. James, in Section 3, points out that the only way to avoid the difficulty of an infinite integral for the total kinetic energy of a Rankine vortex is to assume that the rotational field ceases at some distance S from the centre, in which case

$$K = \tfrac{1}{4}\pi\rho R^2 + \pi R^2\rho \log_e (S/R)$$

per unit height and unit mean wind speed. He thus reluctantly discards the Rankine vortex. Now, curiously enough, there is some evidence that as a rule the rotational field does begin quite sharply (see *Geophys. Mem. Met. Off.*, No. 72, Section 3) at a finite distance from the centre of the depression. Not having gone specially into this matter I can only say that this distance is of the order of 1,000 km, that is, commonly about $2R$. Doubtless ρ and S/R change with time in the life of any given depression.

The second point for mention is the probability that the energy cannot be estimated by any very simple integration with respect to height. Initially the rotational energy may be mainly in the lower half of the troposphere ; in the final stage it may be mainly in the upper part (see *Weather*, **4**, p. 398, 1949).

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7 March 1951

Radiation in the troposphere and lower stratosphere

The very interesting critical review article ' Radiation in the troposphere and lower stratosphere ' by Goody and Robinson in the *Quarterly Journal*, **77**, p. 151, 1951, was so comprehensive that further amplification might seem unnecessary. Yet their treatment of the albedo of clouds missed some essential points, for several of us have attributed to their statements meanings which ought to be corrected.

On the matter of the albedo of clouds, Goody and Robinson (p. 156) say the following :

> ' . . . . It [the total albedo of the earth] might be obtained by extensive observation of the visual and total albedos of various surfaces, of which cloud is by far the most important. (1) A few early observations of the total albedo of cloud by Aldrich (1919) gave a value of 78 per cent, sensibly independent of thickness, and this was accepted by Simpson and others in studies of the heat balance of the earth. More recent observations from aircraft by Neiburger (1949) and Fritz (1950) of the total albedo of various clouds have suggested considerable variation with type and thickness, in agreement with the early work of Luckeish (1919) on the visual albedo, but a preliminary note (*Harvard Report*, 1949) suggests that Hand *has failed to confirm these measurements, and indeed has obtained the same result as Aldrich.* (2) The mean suggested by Fritz [1949] *for all types of cloud* is 50 per cent to 55 per cent, which is so *different from Aldrich's value* that we must hope *for an early resolution of the discrepancy* . . . . '

The numbers (1) and (2) and the italics were not included in the original text, but are added here to facilitate discussion.

(a) Consider the more important item (2) first. No ' resolution of the discrepancy ' is required because no real discrepancy exists ; Aldrich's value, 0·78, and the suggested value 0·50 to 0·55 do not represent measures of the same quantity. Aldrich measured the cloud albedo during *one morning* (17 September 1918) over *one type* of cloud (stratus) near

one place (Mt. Wilson, California). His value should be taken as the mean of one morning's measurements at one point and nothing more. If Aldrich's mean value for that morning, 0·78, should happen to equal the average albedo ' for all types of cloud ' averaged over the whole earth and over time, that would be a remarkable coincidence. By contrast the value 0·50 to 0·55 was based on computations which included the effect of the average cloudiness over the whole earth, and therefore, by definition, estimates the average albedo for all clouds. It in no way involved any albedo measurements from aircraft but was computed from Danjon's average value, 0·39, of the visual albedo of the whole earth, which in turn was based on numerous measurements over a number of years. Although there is good evidence that the true value of the average cloud albedo lies near 0·50 to 0·55, no claim is made here that this is unalterably correct, for this value was based on many simplifying assumptions, and even Danjon's value of 0·39 has not yet been independently verified. What is emphasized here is that the aircraft albedo-measurements of Aldrich, and of others, which have been made up to the present time, represent only a very small sample of cloud albedos and none of these samples can reliably represent the average albedo of all clouds ; but the computation which yielded the value 0·50 to 0·55, regardless of its accuracy, was specifically designed to estimate the average albedo of all clouds. Despite the fact that Aldrich, Simpson, and others have persisted in using Aldrich's single-morning measurement as though it were the average albedo for all clouds, that usage is unjustified and should not be perpetuated. Thus, the discrepancy noted by Goody and Robinson does not exist.

(b) The italicized portion of item (1) in the excerpt from Goody and Robinson intimates that there may be some uncertainty as to whether or not the albedos of individual clouds vary. Even if the albedo of a cloud had never been measured, we could be fairly certain from surface observations alone that the albedo of one type of cloud is often different from the albedo of another type. As an extreme, the albedo of cirrostratus, for example, is surely smaller than the albedo of the top of a cumulonimbus cloud.

Regarding the albedo of a single cloud type, Neiburger found in a long series of measurements that the albedo of stratus varied and that the vertical thickness was one of several important parameters in determining the albedo. There seems to be no reason to doubt this conclusion ; on the contrary, had he found that the albedo was invariant, the result would have been very surprising.

The evidence leaves no doubt that the albedos of clouds vary greatly among cloud types and within cloud types. Consequently, it might well be that all the sets of measurements mentioned by Goody and Robinson in item (1) are individually correct and mutually consistent. The fact that Hand's observation agrees with Aldrich's has no special significance.

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551.515 : 551.509.317

Dynamical methods in synoptic meteorology

The January 1951 meeting of the Society took the form of a discussion on ' Dynamical ethods in synoptic meteorology,' the President, Sir Robert Watson-Watt, C.B., F.R.S., ing in the chair. The openers of the discussion were Dr. R. C. Sutcliffe, Mr. E. J. Sumner d Mr. F. H. Bushby (Meteorological Office, Dunstable).

Dr. R. C. Sutcliffe : In opening the discussion I wish first to draw attention to the le which, so far as I am concerned, will be taken as restricting the field to those dynamical ncepts which have found application in synoptic practice, in forecasting if you like, and excluding some of the more fundamental work on dynamical meteorology which, im- rtant as it may be, has, for one reason or another, whether the abstruseness of the athematics or the obtuseness of the synoptician, so far made little impact upon noptic thinking.

Dynamical ideas are no new thing in synoptic meteorology. When, twenty-odd ars ago, I was first introduced to the subject, convergence and divergence, subsidence and cent, non-geostrophic motions and the like were already familiar ideas to the practising recaster. Bjerknes's circulation theorem for example had appeared with the new century d Richardson's work on ' Weather prediction by numerical process ' was already ailable. At that time however the fruitful ideas of the Norwegian school, on fronts d air masses, were rapidly gaining ground, indeed almost sweeping the field, and however uch we recognize the inspiration of these ideas it must be conceded that their dynamical ntent was vague and unsatisfactory. There were of course other good reasons why namical ideas were rather in the background, especially the one that observational ta from the free atmosphere were so inadequate that dynamical theories were difficult test and virtually impossible to apply in current forecasting.

Whatever importance will ultimately be accorded to recent dynamical methods there ems little doubt that the period beginning shortly before World War II will come to recognized as a new phase in synoptic meteorology characterized by the availability much current aerological data and by the efforts to introduce methods based on quanti- ive dynamical principles. With apologies to other workers, particularly in Germany d in Russia, I shall open the discussion with a brief mention of only two lines of progress, at springing from Rossby's barotropic theory and that from our own treatment of baro- nic development. As the object is to prepare the way for discussion I shall grossly over- nplify and to that extent misrepresent the position but I hope not to any fundamental gree.

Starting on the basis of horizontal non-divergent motion in a barotropic fluid the circu- ion theorem shows that the vertical component of absolute vorticity is conserved. Ignoring the moment changes in the vertical component of the earth's angular velocity (half Coriolis parameter) it is readily seen that a sinusoidal wave form in an otherwise uniform rrent must be translated with the speed of the basic current since the stream-line curvature ich defines the vorticity is conserved. When, however, the current is taken as zonal d variations in the Coriolis parameter are introduced the difference in vorticity from icyclonic ridge to cyclonic trough is partially balanced by the greater cyclonic vorticity e to the earth's rotation at the higher latitudes. The air moving with a northward or thward component can preserve its vorticity and so move without divergence only if change in the Coriolis parameter is balanced by change in curvature. This requires t the air must flow through the pattern and the wave form must move more slowly than current. Instead of $c = U$, Rossby obtained the formula $c = U - U_R$, where U_R is Rossby regressive component $\beta L^2/4\pi^2$. Here c is the speed of the wave form, U the ic zonal current, β the variation of the Coriolis parameter with latitude and L the wave gth.

As a special case the wave form may remain stationary, $c = 0$, when $U = U_R$, *i.e.* en $L = 2\pi(U/\beta)^{\frac{1}{2}}$.

Although the connection between non-divergent motion of a barotropic fluid and atmospheric flow may seem far-fetched, the correspondence between the computed stationary wavelengths and those of the observed quasi-stationary long waves in the westerlies is found to be so good that some element of reality in the model must be suspected. Justification has been provided by the conception of a level of non-divergence in the middle troposphere to which the theory may apply with a close approximation and recently by Charney's conception of an equivalent barotropic flow. Although the flow at no one level may be non-divergent the vertical integral of divergence is only that required to account for surface-pressure changes, a few per cent per day at most, and in some mean sense the flow may be regarded as virtually non-divergent. Work by Charney and others, elaborating the basic idea in various ways, has provided methods which aim to compute the change in ' mean flow pattern ' in this way. Published work to date can hardly be claimed as more than promising but one hears of further progress and optimism is justifiable.

The second line of approach has previously been discussed before the Society and time prevents further elaboration here. However we may note that whereas the ' barotropic ' conception deals with the evolution of the mean flow by inertia the development theory directs attention to the vertical variations in the flow and to the associated divergence, convergence and vertical motion.

As a first approximation to the difference in divergence between the surface and an upper level we obtained the expression

$$l\,(\mathrm{div}_p\,\mathbf{V} - \mathrm{div}_p\,\mathbf{V}_0) = -\,V'\frac{\partial}{\partial s}(l + \zeta + \zeta_0)$$

where $\mathbf{V}$ is the horizontal velocity, $\mathbf{V}' \equiv \mathbf{V} - \mathbf{V}_0$, ζ is the vertical component of vorticity (in isobaric co-ordinates) and $\partial/\partial s$ signifies differentiation in the direction of $\mathbf{V}'$.

If we may assume that the upper level is one of non-divergence, then

$$\frac{d\zeta_0}{dt} = -\,V'\frac{\partial}{\partial s}(l + \zeta + \zeta_0)$$

$$\frac{\partial\zeta_0}{\partial t} = -\,V'\frac{\partial}{\partial s}(l + \zeta + \zeta_0) - \mathbf{V}_0 \,.\, \nabla\zeta_0$$

With the geostrophic approximation to velocities and vorticities $\partial\zeta_0/\partial t$ may be computed and with appropriate boundary conditions the rate of change of the surface geostrophic field may be obtained by numerical integration. Some work has been done on these lines but I am too sceptical of the validity of the idea of a level of non-divergence (and other approximations) to expect the results to have more than qualitative value. The same doubt, incidentally, makes me sceptical of the equivalent barotropic atmosphere as a valid quantitative approximation.

Nevertheless the theory has been found a useful tool in the hands of synopticians and for what it is worth I show the result of one computation of $\partial\zeta_0/\partial t$ compared with the value determined from tendencies. (The example showed a depression to the west of Iceland which moved rapidly south-eastwards down the line of occlusion ; the cyclonic development area was well indicated in this sense at a time when extrapolation of previous tendencies had led to forecasting a slow eastward movement).

Mr. E. J. Sumner : I have two distinct contributions to make to this evening's discussion. The first is on the importance of vertical stability in synoptic developments (Sumner 1950) ; the second sets out some results of a test of the Rossby wave formula and of an attempt to apply it numerically in short- and medium-range forecasting.

1. Vertical stability and synoptic development

The dynamical temperature changes associated with organized vertical motion in the atmosphere automatically produce fields of relative divergence which for vertical stability

ppose and for instability promote both cyclonic and anticyclonic development. The agnitude of the effect, at least for the simple model adopted in the original paper, is rectly proportional to the stability parameter (*i.e.*, the departure of the actual lapse of mperature from the wet- or dry-adiabatic lapse rate, whichever applies) and inversely roportional to the square of the dimensions of the pressure system and to the square of e Coriolis parameter. Small systems would hardly be expected to develop at all in a able atmosphere, especially in low latitudes. For instability in depth, which can only ise in the case of cyclogenesis, the development would proceed very rapidly.

2. A test of the Rossby formula on 500 mb contour charts

(a) This test was concerned with the movement of long atmospheric waves on 500 mb harts, the sole criterion of selection being a well marked wave-like flow pattern (however haped) comprising at least two troughs separated by a ridge. The data were considered s overlapping trough pairs, n successive troughs of a wave train giving $(n-1)$ cases. In eneral, however, there were seldom more than one or two trough pairs within the area onsidered, *viz*, the half hemisphere from 120°W. to 60°E. The period studied was from eptember 1948 to August 1949, the total number of pairs being 172.

The Rossby (1939) formula is $c = U - \beta L^2/4\pi^2$ . . . . (1)

here c is the wave speed, U the mean zonal velocity, L the wavelength and β the rate of hange of Coriolis parameter with latitude. The various parameters of this equation ere measured on 500 mb circumpolar charts in accordance with the following procedure, he zonal parameter being determined in several ways.

In order to locate the wave train as a whole a latitude was selected (to the nearest 5°) hich was central to the pattern. The longitudinal positions of the troughs and ridges with espect to this datum latitude were determined by where the axes (defined as lines joining he lowest or highest latitude of individual contours of the troughs and ridges, respectively) ut the selected latitude. An example of an actual wave pair showing the axes and the elected latitude is given in Fig. 1.

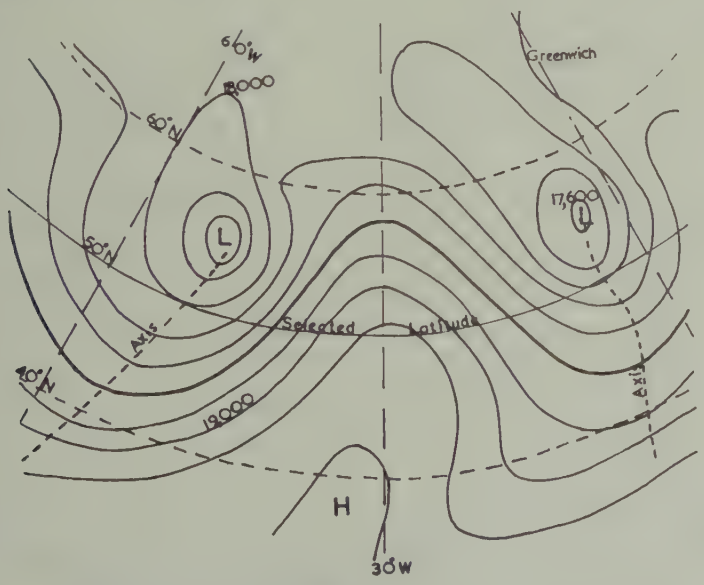

Figure 1. Showing the axes and selected latitude of an actual wave pair.
The diagram is part of the 500 mb circumpolar chart for 3 Sept. 1948 (0300 GMT).

In addition to these measurements the individual troughs were classified as tilted or not, the precise direction of tilt being noted, and their amplitude measured in accordance with a definite procedure.

The Rossby wave formula requires some measure of U, the basic zonal velocity, L the wave length and the parameter β. The latter was taken as the appropriate value at the selected latitude, and L was defined as the trough-to-trough distance between the points where their respective axes cut the selected latitude. The determination of the value of U presents more difficulty. In the simple model originally used by Rossby this parameter is the mean zonal flow over any width containing the wave since the basic current is constant over the entire field. However, atmospheric waves are always finite in width and have some lateral shear, so that averaging gives different values for different zonal widths. The value of U was therefore determined in a number of different ways in order to decide which gave the best results under test.

The first two parameters computed, U_1 and U_2, were respectively given by the mean zonal flow (expressed as the geostrophic equivalent of the mean pressure gradient) within the two bands 10° and 20° of latitude in width, *i.e.*, 5° and 10° on either side of the selected latitude. These values were computed only for the restricted sector comprising a particular pair of waves.

Since the selected patterns often contained closed centres, a certain amount of easterly flow occurred which effectively reduced the values of the above parameters, particularly U_2. It was therefore decided to use certain additional indices, following the main flow round the troughs and ridges. A line judged to be more or less central to the flow was followed and pressure differences for 10° and 20° ranges of latitude were read off at given points along the profiles. The geostrophic equivalent of the mean of these readings determined the two indices U_3 and U_4, respectively.

(b) Estimates of the 24-hr displacements of the waves, using Eq. (1) and all the different zonal parameters in turn, were correlated with the actual subsequent 24-hr displacements of the upwind and downwind troughs of particular trough pairs separately, and with the average displacements of both troughs of a pair, the data being segregated into seasons. The corresponding regression coefficients were also worked out.

It is not proposed to show these results in detail, but the following points and conclusions may be mentioned : (i) The correlation coefficients ranged in value from about 0·20 to about 0·80, being appreciably higher for the cooler seasons (winter and spring) than for the warmer seasons. They also showed a large increase in value for the downwind as compared with the upwind troughs. (ii) Under the same circumstances of season and type of trough (upwind or downwind) the differences between the correlations for the various zonal parameters were small. This was partly due to the fact that none of the zonal indices computed made much contribution to the success of the formula, as was evident when the appropriate partial correlations were computed. (iii) In all cases the simple Rossby formula had to be modified by means of regression coefficients which varied seasonally and, more especially, with the particular zonal index used. (iv) The formula was considered to have no practical forecasting value in the summer of for the upwind member at any season, the particular zonal and wavelength parameters used here being more appropriate to determine the speed of propagation of the downwind troughs.

(c) All these results are vitiated to a certain extent by errors of a random nature, among them errors of drawing and positioning the wave patterns. A rigorous test of the Rossby formula requires that these errors should be eliminated or, at least, effectively reduced. Also the importance of the individual parameters in the equation must be considered.

In order to smooth out the various errors as much as possible a procedure similar to that of Cressman (1948) was adopted. Occasions on which a particular wave pair was conserved for three days were chosen and the average displacements of the troughs in ° long./day were correlated with the averages of the three daily estimates (using U_2 for convenience).

Preliminary calculations using this smoothing technique showed a definite increase

the values of the correlation coefficients at all seasons. For the mean trough displacement, example, the correlation for the cool seasons collectively (38 cases) was 0·82, and 0·63 the warmer seasons (61 cases). The partial correlations (cool seasons) were as follows : an displacement with U_2, 0·32 ; mean displacement with the expression βL^2, — 0·85. he lowness of the first partial correlation confirms the lack of importance of these zonal rameters (sub-section b(iii)) in determining the wave velocity. However, it seemed so likely *a priori* that the strength of the zonal current was of such little significance that it s decided to repeat these computations making a more accurate determination of the ntral latitude.

The individual daily values for the 38 overlapping 3-day periods (cool seasons) were refully recomputed, the selected latitudes now being to the nearest whole degree. The nal parameters U_2 were calculated with respect to the new datum latitudes and for a 20° nd as before. The results of this work are set out in Table I. The required correlation the smoothed data is designated by r_2 ; the partial correlations ρ_1 (actual displacement th U_2) and ρ_2 (actual displacement with βL^2) are also shown. The coefficients (a, b) e for the regression of the actual displacement on the Rossby estimate in the form $= a(U_2 - \beta L^2/4\pi^2) + b$, where A is the best fit with actuality which can be obtained r a given estimate ; the units of b are ° long. /day. The last column contains the corres- nding correlations (r) based on all the 64 daily values making up the 38 3-day periods.

ABLE I. SHOWING THE CORRELATIONS AND PARTIAL CORRELATIONS FOR DATA BASED ON 3-DAY AVERAGES ; COOL SEASONS ONLY

ough	r_2	a	b	ρ_1	ρ_2	r (based on daily values)
pwind	0·72	0·52	4·1	0·26	— 0·71	0·40
wnwind	0·90	0·76	2·2	0·64	— 0·82	0·65
ean displacement	0·93	0·64	3·1	0·65	— 0·90	0·71

Except for the upwind trough, there is a noticeable increase in the values of r_2 compared th the previous results quoted above which seems to be effected mainly by the presumably ore representative determination of the zonal index. The increase in ρ_1 is particularly table ; although still smaller than ρ_2 it now represents a useful contribution to the success the formula. The average departure of the actual displacements from the line of best is 1·4 ° long. /day for the mean trough displacements, which is well within the limits error of drawing and measurement. (An actual check showed that the average error e to these causes was about 2-3 ° long. at all seasons).

The smallness of both partial coefficients for the upwind trough is perhaps what one uld expect. The simple type of interaction envisaged by the Rossby theory is a down- nd effect, the pressure field progressively adapting itself to the field of motion while e individual air particles follow constant vorticity trajectories. The very low value of for the upwind member implies that it is the zonal index rather than the wave length at is least representative of conditions further upwind on which its rate of propagation ay be expected to depend.

These latter results were repeated using a zonal parameter for a 30° band symmetrical the datum latitude. However, there was no significant difference between the values nd and those given in Table I, which in view of previous results suggests that within asonable limits there is no optimum width over which one should mean the basic zonal w in order to achieve the best results.

An attempt was also made to assess the importance of the parameter β in the formula. r these data, however, very little significance was found, possibly owing to the smallness

of the variation of β over the range of latitude met with (27 of the 38 cases lay between 50° and 59°N., for example).

(d) The general importance of the zonal velocity and the wave length (especially the latter) in determining the speed of atmospheric waves has been established by the above results. Treating U_2 and βL^2 as independent variables (for the 38 smoothed data, downwind troughs only) the following multiple regression equation was obtained :

$$A = 0{\cdot}80\ U_2 - 0{\cdot}72\ \beta L^2 + 1{\cdot}0,\ ^\circ \text{ long./day}$$

The appropriate partial correlation coefficients are given in Table I. It will be noticed that both terms have to be reduced by different numerical factors. In view of the possible effect of width, lateral shear, the tilt of the axes (affecting the second term only) and of departures from the equivalent-barotropic level (affecting U_2, Charney and Eliassen 1949) a certain amount of individual modification of the terms of the Rossby equation is to be expected. Accordingly an attempt was made to assess the importance of various likely parameters on the movement of these waves. This was confined to the winter and spring data and to the downwind troughs only. However, the results were inconclusive and difficult to interpret.

There were some positive indications that waves with tilted axes tend to move faster than waves with axes more or less parallel to the meridians, other things being equal, which is in keeping with Machta's (1949) theory, but the magnitude of the effect was such that no absolute significance could be attached to the results.

The possible effect of lateral shear of the mean zonal current was not in evidence at all, possibly owing to a systematic compensation by the width parameter (see Thompson's formula, 1948). It was felt that a more specific test than we could attempt was required in order to assess the importance of lateral shear and width of the wave disturbance which would be very difficult in practice since it is almost impossible to delineate the precise boundaries of a particular wave train. However, it is as well to bear in mind that such factors as the above, apart from being of great importance in particular cases, may be sufficiently variable over longer periods and from one set of data to another to alter radically the values of the regression coefficients in an equation which ignores them.

The effect of amplitude or of simple 'confluence' or 'difluence' of the basic zonal current was not in evidence in these data. The lack of effect of amplitude conforms with Neamtan's (1946) demonstration that Eq. (1) does in theory apply to waves of any amplitude.

It is concluded from the above that in so far as the movement of long atmospheric waves is determinable on the basis of the equivalent, barotropic assumption, the appropriate model is by no means a simple one. However, there is some justification for believing that in certain cases and for limited periods of time a useful first approximation can be obtained on this assumption provided all aspects of the flow are considered, *e.g.*, by numerical integration from observed streamline patterns in the manner suggested by Charney (1948).

(e) A direct comparison with the routine results obtained by the Forecasting Division of the Meteorological Office, Dunstable for the same period as for this investigation suggested that the modified Rossby formula improved slightly on present short-range forecasting methods provided regression coefficients obtained from the data themselves were used. However in order to assess the practical value of the above work a test on independent data is necessary.

The period chosen was from January to May 1950, and only troughs that were preceded by another (*i.e.*, downwind troughs) were selected (46 cases in all), the datum latitude being to the nearest whole degree. The regression coefficients it was thought best to use were those for U_2 based on the 64 daily data comprising the 38 3-day periods, viz., $a = 0{\cdot}69$, $b = 2{\cdot}3$.

The results for the Rossby formula were now found to be rather worse than the routine forecasts, which after the event was partly attributed to our having used inappropriate regression coefficients. For the data in question, the line of best fit was obtained by

putting $a = 0{\cdot}57$ and $b = 0{\cdot}1$ compared with 0·69 and 2·3 actually used ; the corresponding correlation was 0·66, which is as high as any obtained for similar data.

A similar direct comparison with medium-range forecasting (3-day prebaratics) is not possible since such forecasts are only done twice a week in the Research Division, Dunstable, which periods do not often correspond with these for which wave trains persisted throughout. Actually, within the period in question, there were only six cases of a wave train persisting the entire three days of the forecast. For these cases the experimental forecasts were much better than the estimates based on the formula but the number of cases is too small to permit of a conclusive comparison.

In order to increase the number of cases the same period was reviewed without being tied to forecast periods, and 25 3-day periods (some of them over-lapping) were found. The average error using the modified Rossby formula and the *initial* wave length and zonal parameters (the appropriate regression coefficients for such a procedure having been determined from the corresponding results of the previous data) was 13·0° long., the corresponding mean 3-day displacement of all the (downwind) troughs concerned being 20° long. These results are considered to be too poor to justify the use of such simple regression formulae in medium-range forecasting. Here again the regression coefficients determined from these 25 cases after the event were very different from those actually used and previously determined.

It is concluded from these results that no particular improvement on the standard of accuracy at present attained by routine forecasting techniques can be expected by an application of the modified Rossby equation. If regression coefficients based on a much longer period were available, and if in addition some segregation (*e.g.*, into simple or tilted wave forms) were attempted, it is very probable that the above comparison would be improved (*i.e.*, in favour of the formula) but it is by no means certain that the improvement would be sufficient to better the routine results.

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Mr. F. H. Bushby : The previous speakers have dealt with certain aspects of the current theoretical approaches to the dynamical changes which occur in the earth's atmosphere ; the equivalent barotropic atmospheric theories which have been mainly developed in the United States by Rossby, Charney and others, and the ' development ' theory of Sutcliffe (1947). I have been concerned with the use of numerical methods in forecasting and have done a certain amount of work along each of these different lines of approach. Two of the subjects on which I have been working are, I think, particularly relevant to the present discussion. One of these is an investigation into a numerical method proposed by Charney and Eliassen (1949) for the prediction of 500 mb contour maps, and the other is an attempt to solve numerically particular cases of the ' vertical stability ' equation derived by Sumner (1950).

.. Charney and Eliassen's formula for prediction of changes in the 500 mb contour heights

Recently, Charney and Eliassen (1949) have given a numerical method of computing changes in the 500 mb contour height. The method is essentially one-dimensional and depends upon the following main assumptions :

1. The existence of an equivalent-barotropic level at approximately 500 mb.
2. That the motion can be considered as consisting of small pertubations superimposed upon a west-east zonal current constant with respect to time and longitude.
3. The height perturbation of the 500 mb level is assumed to have a sine dependency on the north-south co-ordinate.
4. The shape of the streamlines is the same at all levels and the increase of wind with height is the same along all verticals.

Synoptic meteorologists know that many situations occur when these assumptions do not hold. Nevertheless, Charney thought that by treating the atmosphere in this way he might obtain a first approximation to the actual behaviour of the 500 mb level.

Briefly, Charney commences with the well-known Rossby vorticity equation which relates changes of vorticity to the divergence in a horizontal layer of the atmosphere. Averaging vertically with respect to pressure and using the tendency equation, he shows (with some approximation which is not easy to justify) that there is one level in the baroclinic atmosphere at which the flow corresponds to motion in a barotropic fluid. This level is called the equivalent-barotropic level, and is shown to be between 500 and 600 mb. In practice, 500 mb is used, as this is a level at which routine contour charts are drawn.

By considering the motion at this level as small perturbations on a uniform west-east zonal current, utilising the geostrophic approximation for vorticity, and introducing one or two refinements due to the effects of topography and friction (which are later neglected in a 24-hr forecast), Charney obtains the following linear differential equation for z, the height of the 500 mb contour level.

$$\left(\frac{\partial}{\partial t} + U\frac{\partial}{\partial x} + \mu F\right)\left(\frac{\partial^2 z}{\partial x^2} - m^2 z\right) + \beta\frac{\partial z}{\partial x} - \lambda^2\frac{\partial z}{\partial t} = -\mu\lambda^2 U\frac{\partial h}{\partial x} \quad . \quad . \quad (1)$$

where x and y are the space variables in the east and north directions respectively (x being measured in degrees of longitude) ; t is the time variable (measured in days) ; U is the zonal current ; u and v are components of velocity in the x and y directions respectively ; $\mu = u_0/u$ where suffix 0 refers to the top of the friction layer ; $F = \dfrac{\sin 2\alpha}{H}(kl/2)^{\frac{1}{2}}$ where α is the angle between the isobars and the surface wind, K = eddy diffusivity, l is the Coriolis parameter, and H is the height of the homogeneous atmosphere ; m^2 is given by the relationship $\partial^2 z/\partial y^2 = -m^2 z$; $\beta = \partial l/\partial y$; $\lambda^2 = l^2/gH$ and h (x) is the mountain profile along the latitude circle in question.

Eq. (1) can be solved for z by Fourier analysis. The boundary conditions used are the initial values of z, and the solution can finally be written in the simple form

$$z(x + Ut, t) = z(x, 0) + \sum_{N=-3}^{7} A_N\, z(x + 10N, 0) \quad . \quad . \quad (2)$$

The coefficients A_N can be calculated from the constants explicit in Eq. (1). Physically, the mathematical treatment is equivalent to analysing the actual 500 mb flow pattern into a series of wave components each of which is then moved on with its own wave velocity.

Two particular cases of the 500 mb flow pattern to which Charney's formula was applied are shown in Figs. 1 and 3. In each case the forecast was for a period of one day from longitude 80°W. to 80°E. along the 45°N. latitude circle. Comparisons of the forecasts with actuality are shown in Figs. 2 and 4. It can be seen that at 0300 GMT on 2 Nov. 1949, there were considerable variations in the strength of the zonal current over that portion of the chart for which a forecast was computed. This probably accounts for the lack of agreement between the computed change and the actual change. At 0300 GMT on 8 Nov. 1949, the 500 mb flow could reasonably be regarded as small perturbations on a west-east zonal current, apart from the strong south-north flow between 20° and 40°E., and it can be seen that this is the only part of the computed forecast which did not agree tolerably well with actuality.

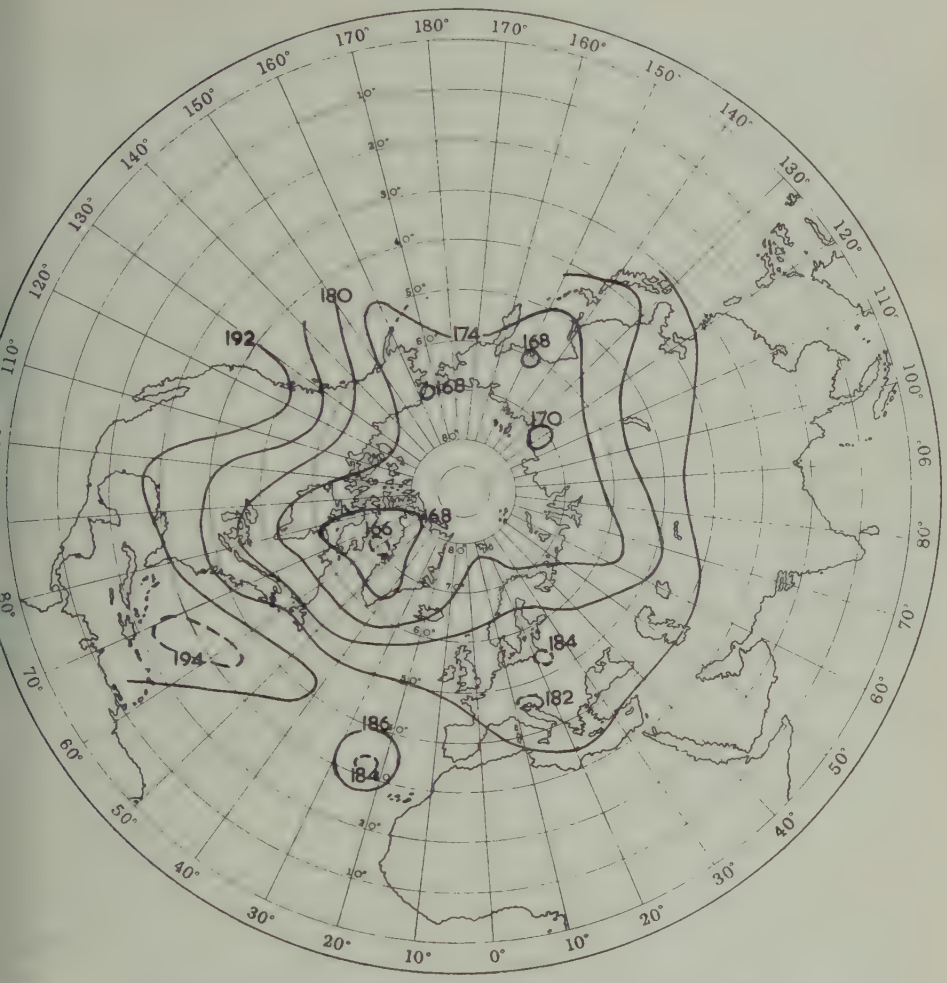
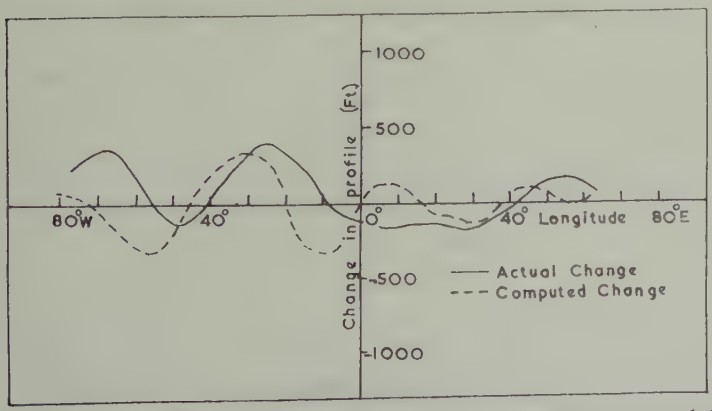


Figure 1. 500 mb contour pattern at 0300 GMT on 2 Nov. 1949.



gure 2. Comparison between the computed and actual 24-hr change in the 500 mb height profile along latitude 45 N. from 0300 GMT on 2 Nov. 1949.

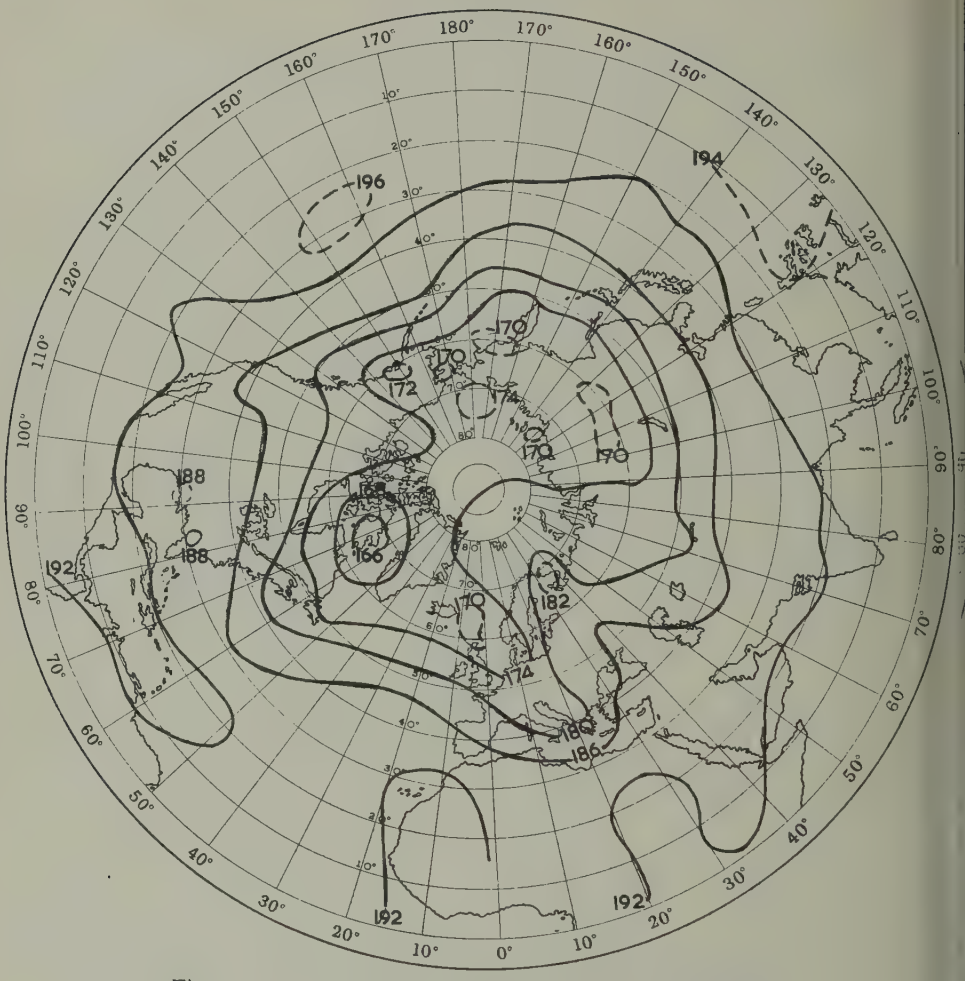


Figure 3. 500 mb contour pattern at 0300 GMT on 8 Nov. 1949.

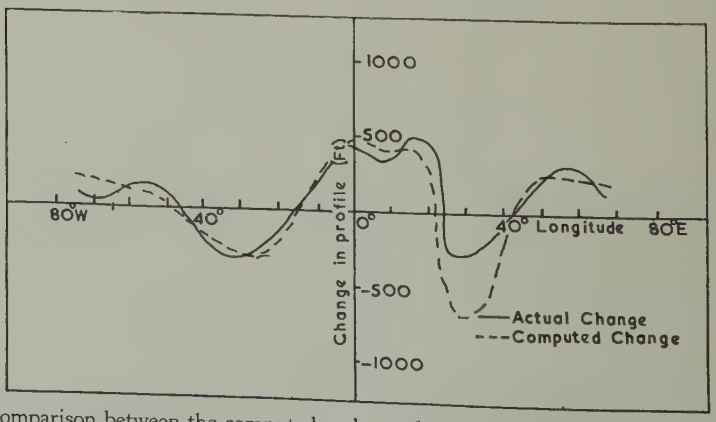


Figure 4. Comparison between the computed and actual 24-hr change in the 500 mb height profile along latitude 45 N. from 0300 GMT on 8 Nov. 1949.

Two series of tests have been carried out in the Forecast Research Division, Dunstable n the accuracy of the forecasts produced by this method at various latitudes between) and 60°N. Each series of tests considered forecast profiles on about 30 days. Two pects of the forecast were compared with actuality. The forecast changes in spot heights the 500 mb contour surface were compared with the actual changes, and the forecast ovements of trough and ridge lines were compared with their actual movement. The sts showed fairly conclusively that the results obtained from Charney's formula were ot quite so good as those obtained by conventional methods at Dunstable. However, hen forecast charts were prepared using profiles obtained from Charney's formula in titudes 40, 50 and 60°N. simultaneously, the forecast movements of the trough and ridge nes at 50°N. were slightly better than those forecast on the routine chart. The spot eights, however, were less accurate.

It can be seen from Eq. (2) that the change in the 500 mb height forecast by Charney's rmula consists of two essentially independent parts ; the first representing a displacement the existing profile eastward with the zonal current, and the second representing the ntribution of various terms in which the variation of the Coriolis parameter with latitude ays an important part. The question arises as to whether the second set of terms do ntribute significantly to the success of the method. In order to investigate this, corres- nding forecasts were made on the basis of the first term only, *i.e.*, on the assumption at the 500 mb profile moves eastward with the zonal current. These forecasts were much ss accurate than those produced by Charney's formula.

The results of these series of tests lead one to the conclusion that Charney's formula es not yield results good enough to enable his method to be introduced into routine recasting, but nevertheless the method does show significant success. I do think it nportant, however, that Charney has been able to derive a simple arithmetical formula r forecasting the 500 mb contour heights, even though the results obtained are not quite good as those obtained by conventional methods.

Charney (1949) realised the limitations of this one-dimensional method, and with a ightly different approach has obtained the following two-dimensional linear differential quation for the instantaneous height tendency at 500 mb.

$$\nabla^2 \frac{\partial z}{\partial t} = J(\zeta + l, z) \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where ∇^2 is the plane Laplacian

J is the plane Jacobian

ζ is the vertical vorticity component of the atmosphere at 500 mb relative to the earth.

The right hand side of Eq. (3) can be evaluated at a finite number of points, and the quation can then be solved numerically by ' relaxation methods,' (using the boundary ndition $\partial z/\partial t = 0$ near the equator). Two cases have been solved at Dunstable, but e results have in general been discouraging when compared with actuality. This is obably due partially to the fact that baroclinity has been neglected in deriving Eq. (3) d partially due to the fact that over large areas of the northern hemisphere the analysis the 500 mb chart is doubtful owing to lack of observations.

One of Charney's assumptions is the essentially barotropic idea that the shape of the reamlines is the same at all levels and that the increase of wind with height is the same ong all verticals. This can be expressed mathematically as

$$u = A(p)\,\bar{u}\ ,\ v = A(p)\,\bar{v} \quad . \quad . \quad . \quad (4)$$

here the bar denotes the mean taken vertically with respect to pressure.

At Dunstable, we have made the more realistic assumption that

$$u = \bar{u} + B(p)\,u'\ ,\ v = \bar{v} + B(p)\,v' \quad . \quad . \quad (5)$$

here u' and v' are the x and y components respectively per unit thickness of the mean

wind shear through the troposphere. By a similar analysis to Charney's we get an additional correction term on the right hand side of Eq. (2). Results so far obtained seem to indicate that this correction term provides a correction of the right sign but of a greater magnitude than is required to make the forecast accurate, but we are still working on this problem.

2. Computation of vertical velocities

The second subject on which I have been working recently is the computation of the mean vertical velocity in the 1,000-500 mb layer taking account of vertical stability. Sumner (1950) has modified the 'development' equation of Sutcliffe (1947) so that the effect of 'vertical stability' can be considered. Thus for a layer of the atmosphere between two pressure levels p_0 and p_1 we can write

$$l\,(\mathrm{div}\,\mathbf{V}_1 - \mathrm{div}\,\mathbf{V}_0) = -V'\frac{\partial}{\partial s}(l+\zeta_0+\zeta_1) - \frac{R}{l}\log_e\frac{p_0}{p_1}\nabla^2\,\overline{(\Gamma w')} \qquad . \qquad (6)$$

where $\mathbf{V}_0$ and $\mathbf{V}_1$ are the horizontal wind vectors and ζ_0 and ζ_1 the corresponding vorticities at the levels p_0 and p_1 respectively ; $\partial/\partial s$ represents differentiation in the direction of the wind shear vector $\mathbf{V}'$; R is the gas constant ; Γ is the departure of the actual lapse of temperature ($-\partial T/\partial z$) from the appropriate wet or dry adiabatic lapse rate ; w' is the vertical velocity relative to a moving isobaric surface, and the bar represents the vertical mean with respect to a logarithmic pressure scale.

The following assumptions were made :

1. That there is a linear variation of divergence with pressure, passing through a level of non-divergence at about 600 mb (mid-troposphere).

2. That Γ and $\overline{T}$ (the mean temperature of the layer) can be assumed constant.

3. That variations in the Coriolis parameter are small compared with those of the relative vorticity.

We can then write Eq. (6) as

$$\nabla^2\,\overline{w'} + a\,\overline{w'} + G\,(x,y) = 0 \qquad . \qquad . \qquad . \qquad (7)$$

The constant a can be computed, and $G\,(x,y)$ can be calculated at a finite number of points. Eq. (7) can then be solved for $\overline{w'}$ by the relaxation technique.

Fig. 5 shows the synoptic situation at 1500 GMT on 14 Mar. 1949. Fig. 6 shows the field of mean vertical velocity in the 1000-500 mb layer corresponding to this situation when the effect of vertical stability is neglected, and Fig. 7 shows the damping effect of vertical stability. By comparing Figs. 6 and 7 it can easily be seen that the maximum and minimum computed values of vertical velocity have been reduced to about one-third of their magnitude by the consideration of vertical stability, although this has had little effect on the positions of the maxima and minima.

In deriving Eq. (7) from Eq. (6) it is difficult to justify the assumption that Γ is constant over the field. The next step we intend to take is the consideration of variations in Γ.

Bannon (1948) has derived in tabular form a relationship between the mean vertical velocity in a layer of the atmosphere and the rate of rainfall. From Fig. 7 it can be seen that the maximum upward vertical velocity is about 4 cm/sec. This is equivalent to a rate of rainfall of 1·2 mm/hr. The observed rate of rainfall over the Midlands and southern England on this particular occasion varied between 1 and 2 mm/hr. This particular situation is the only one so far for which Eq. (7) has been solved. However, more cases are being considered to see if there is any correlation between the rate of rainfall equivalent to the mean vertical velocity computed from Eq. (7) and the actual rate of rainfall.

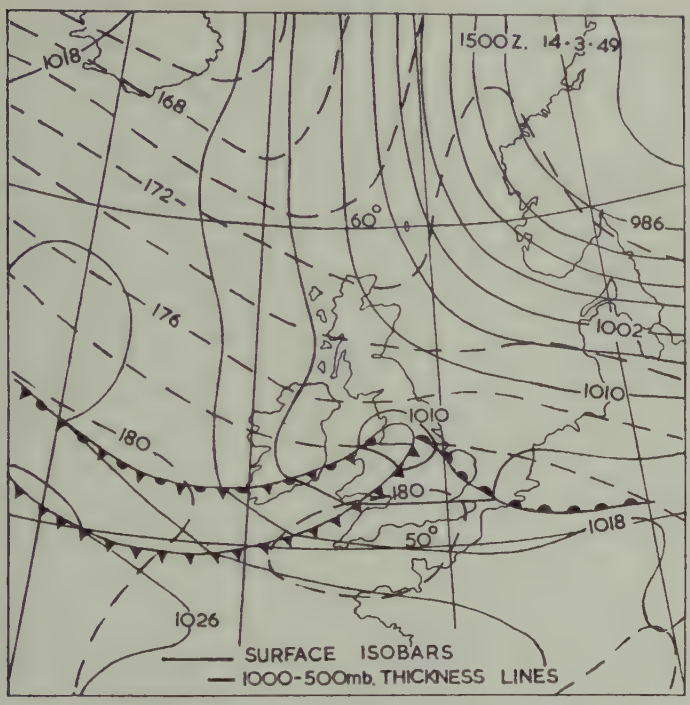
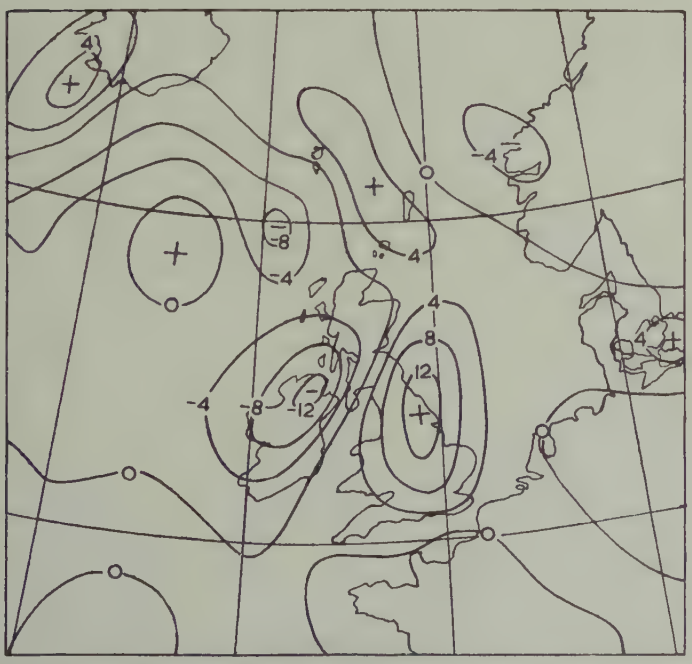


Figure 5. Synoptic situation at 1500 GMT on 14 Mar. 1949.



gure 6. Isopleths of the undamped mean vertical velocity in the 1000-500 mb layer in cm/sec as computed from the synoptic situation at 1500 GMT on 14 Mar. 1949.

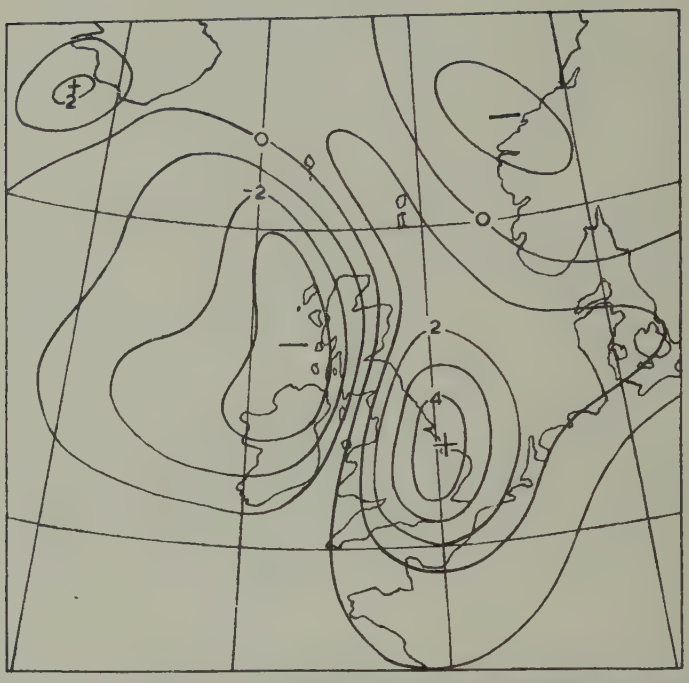


Figure 7. Isopleths of the damped mean vertical velocity in the 1000-500 mb layer in cm/sec as computed from the synoptic situation at 1500 GMT on 14 Mar. 1949.

3. Conclusion

Some of the work about which I have been speaking is still in an incomplete state, but I hope I have been able to show that some progress is being made towards developing numerical methods which will be capable of solving some of the dynamical problems which confront the meteorologist. In spite of this recent encouraging progress, however, there still remains a considerable amount of research work to be done before numerical methods will be able to be applied on a routine basis to weather forecasting.

References

Bannon, J. K.	1948	*Quart. J. R. Met. Soc.*, **74**, p. 57.
Charney, J. G.	1949	*J. Met.*, **6**, p. 371.
Charney, J. G. and Eliassen, A.	1949	*Tellus*, **1**, p. 38.
Sumner, E. J.	1950	*Quart. J. R. Met. Soc.*, **76**, p. 384.
Sutcliffe, R. C.	1947	*Ibid.*, **73**, p. 370.

Dr. R. S. Scorer : The barotropic model of the atmosphere is defective in that it permits no development of disturbances but can only transmit disturbances whose energy comes from some outside source. A model could only be described as being the 'equivalent' of the real atmosphere in the sense that it has the same transmissive properties, but it is then quite useless because the type of disturbance that it is capable of transmitting in this way never occurs, with one exception – waves in the lee of mountains – which are waves stationary relative to the ground. Prof. Rossby's formula for the wave velocity can therefore only be applied when the velocity is zero, when it gives the wavelength in terms of the stream velocity. It is not applicable to moving disturbances ; and I think Prof. Rossby would agree with this statement. Still less is it applicable to depressions, which because of their closed circulations cannot be described as wave (*i.e.*, oscillatory) motions.

The reason why none of these waves that can be transmitted, in theory, through the mosphere as a medium, appear is because there is no outside source of energy to set them . The actual meteorological disturbances are of a completely different nature from ves.

Even supposing that wave theory did describe the actual motion of moving disturbances the atmosphere there is nothing at all to be gained by applying formulae derived for a rotropic model to obtain a forecast because all it can do is to move on disturbances at a nstant velocity, and can therefore give no better result than linear extrapolation and is ich more trouble. Furthermore it makes no use of previous charts and I can see no point natever in denying oneself such help.

I have suggested to Dr. Charney that second, third, or even higher order extrapolation, operly applied, must necessarily lead to better results than can be obtained by use of barotropic model, but he described such a method as 'antediluvian.' Whether or not was the method employed by our Neanderthal ancestors* (and it probably was) it is ll better than anything that can come from the barotropic model. I do not advocate its e (though it is obviously the simplest way in which modern calculating machines could employed to operate upon the data) because I think we can do better : good forecasters eady do as well, and I think the lines upon which Dr. Sutcliffe has been working will be ore productive. No refinements, like those of Machta and Thompson, to the theory can of any avail if they are applied only to the essentially defective barotropic model.

The importance of static stability is not newly-won knowledge. The studies of wave otions by Queney, Charney, Eady and Scorer have shown how important the static ability is ; and how it dominates smaller disturbances but has less effect in the larger sturbances. I do not like Mr. Sumner's description of it as a 'damping' factor. There ems no adequate reason for departing from the accepted practice in mathematical physics calling only dissipative agencies 'damping' factors. They are generally equivalent to e first order term in a second order linear differential equation. The static stability is more a damping factor than the earth's rotation.

Dr. A. G. Forsdyke (*in reply to Dr. Scorer*) : I do not think that Mr. Sumner or deed any other workers in this field regard the motion to which the Rossby formula plied as waves in the usual sense of energy-propagating disturbances. The 'wave ttern' is simply a state of motion required by constancy of absolute vorticity and is scribed as a wave only on account of its shape. The test of the Rossby formula described Mr. Sumner is simply to see if the formula or some simple modification of it is of practical recasting value.

Mr. J. L. Galloway : I should like to ask Dr. Sutcliffe what gain in time over appreciation of the observed changes in surface pressure do the upper-air patterns give the forester. For example, when the upper-air patterns indicate that surface cyclogenesis or anticyclogenesis is likely to occur, at what stage, concurrent or otherwise, are the significant anges of surface pressure apparent ?

Mr. R. P. Pearce : I would like to make a few remarks regarding the general approach this problem. It seems that so far no attempts have been made to describe atmospheric otion in terms of its fundamental causes. We appreciate that cyclones are part of the echanism whereby, in our hemisphere, air heated at the equator is transported northrds, and air cooled in polar regions southwards. In other words differential heating is fundamental cause in terms of which we should calculate the motions of the atmosphere ve are to understand them completely. To use methods of the kind we have heard about s evening, which assume the wind and temperature fields to be to a large extent indendent is to over-simplify and perhaps falsify the problem ; for although applications such methods provide valuable knowledge of how the atmosphere behaves, they cannot d to any complete understanding.

With this in mind I have been studying a small-scale example of differential heating – sea breeze, and attempting to develop a method of approach which enables both the

* The use of the word 'ancestors' is not to be taken as indicating any opinion whether *homo sapiens* superseded or ved out of Neanderthal man.

velocity and temperature fields to be expressed in terms of the heating function and boundary conditions only. It is hoped that this method will indicate an approach along the same fundamental lines to the wider problem of cyclonic development.

Such a quantitative treatment is possible. Its application so far is making it appear quite evident that atmospheric motion generated by differential heating represents a readjustment of a kind which is far from being linear or of wave-like character. The form of the velocity field continually changes with time. The cyclone represents the same fundamental state of affairs as the sea breeze, though on a different scale. The horizontal gradient of the heating function is much smaller, and heating extends to a larger depth of atmosphere. The boundary conditions are much the same. Hence it may be that by treating the cyclone in this way, new light will be thrown on the many problems, such as the existence of fronts and the primary factors determining rate of development, which are fundamentally so little understood.

Mr. E. J. Sumner (*in reply, communicated*) : I find some of Dr. Scorer's remarks obscure, but I agree with what I take to be his general standpoint, *viz.*, that the barotropic assumption cannot provide the ultimate answer to all our problems.

The inadequacies of barotropic theories have been fully realised by their exponents. For example, Rossby (1939), in his classical paper, referred to the absence of 'any available energy supply,' thereby prohibiting the further development of the barotropic perturbations under consideration. There can of course be a redistribution or ' dispersion ' of existing energy in a barotropic medium. Moreover, a model often adopted is based on barotropic flow *plus* sources and sinks of energy, the latter being applied in any convenient way in order to simulate real atmospheric processes.

The question of how good a first approximation such partial theories give can only be finally answered by means of an empirical test. The investigation described above gives some of the facts, but there is still very much more scope for further testing.

Incidentally, I am not aware that the Rossby formula has ever been applied to the movement of depressions as such ; as is well known there is no exact one-to-one correspondence between the long atmospheric waves of the middle and upper troposphere (with which the theory is concerned) and surface-pressure systems. Also there is no doubt that Rossby waves can in theory be moving waves ; whether they are ' equivalent ' in any sense to the (moving) long waves of the upper atmosphere is perhaps more debatable. As I have shown, the individual parameters of the Rossby formula were certainly of some significance in determining the speed of propagation of the troughs considered, which I venture to suggest argues in favour of the theory. I think it should also be pointed out to Dr. Scorer that at present any numerical estimates based on a formula should and would be used in association with all other synoptic aids, including previous charts.

In connection with my paper on *Vertical stability in synoptic development*, some reference to the antiquity of the idea is made therein. The importance of vertical stability is, of course, not new in meteorology, but it needed to be brought out in this particular context and in a form suited to numerical computations.

Dr. R. C. Sutcliffe (*partly communicated later*) : In reply to the discussion may I say that I agree broadly with Dr. Scorer except that I do not think that one need be so particular about terminology. To say that vertical motion is ' damped ' by vertical stability is common usage and to call a wavy distortion of a flow pattern or of a front a ' wave ' is so firmly established in synoptic language that protest is hopeless. While it may be true that topography is the only agent available to set up barotropic disturbances it does not follow that the evolution of a flow pattern set up by baroclinic development processes cannot be in part controlled by inertia processes analogous to barotropic evolution. However I am inclined to agree that when a tolerably satisfactory solution to the three-dimensional problem emerges it will derive little or nothing from the barotropic model – which is literally sterile. The essentially baroclinic solution already published by Charney seems to me to be rather deliberately hung on the barotropic peg and it might have hung just as well if this peg had not existed.

Dr. Scorer's remarks on extrapolation are interesting and I fully agree that the ancient

se of a tool (which still does more useful work than any other) is no argument for discarding
However I am not so sure that extrapolation is fundamentally different from any numerical
tegration of the differential equations by finite difference methods – to which all the
ethods eventually reduce in practical synoptics. The difference is that prediction by
mple extrapolation works with one dependent and one independent variable (time)
nd is valid only so long as the finite difference to be predicted is given with sufficient
ccuracy by the observed past trend. All numerical integrations depend on similar restric-
ons and the fact that physical relations between space and time derivatives are introduced
oes not necessarily mean that the computed prediction is thereby more accurate ; every-
ing depends on the problem and on the nature of the data. It would seem obvious that
o formula such as I have quoted is likely to enable us to calculate the rate of change in
ie surface-pressure pattern as accurately as we can estimate it directly from the observa-
ons. In fact however it is found that observations are often insufficient to define the
ndency field unambiguously and a confirmation from development theory is most welcome.
Moreover it is sometimes possible to predict (by extrapolation methods essentially) situa-
ons which will give rise to new developments not otherwise indicated. I entirely agree
iat a method of numerical computation which deliberately rests only on the latest data and
nores the equally reliable data from the recent past cannot have reached finality, but
iese are early days and I have no doubt that if the present lines of attack show sufficient
romise steps can be taken to redress the omissions. Indeed in routine work where new
redictions must be made at regular overlapping intervals continuity will necessarily be
iven weight and I do not think there is any need to regard the present approaches as on
ie wrong lines merely because continuity through the past is not explicitly carried forward.

Mr. Galloway's question is connected with the above remarks. The development
ieory merely shows that cyclogenesis or anticyclogenesis should be taking place at the
me for which patterns apply. With current charts this may be merely confirmatory of
ie obvious (if the theory is wrong it will confuse with false advice) but when the fore-
aster is considering situations given by forecast charts for a day or more ahead the theory
ould be used to give a check on consistency and to suggest developments which might
ot otherwise have been envisaged.

DISCUSSION

Recent work on the stratosphere

The February 1951 meeting of the Society took the form of a discussion on 'Recent work on the stratosphere,' the President, Sir Robert Watson-Watt, C.B., F.R.S., being in the chair.

The openers of the discussion were: Sir Charles Normand who spoke on 'Some recent work on ozone'; Dr. R. Frith described some recent work of the Meteorological Research Flight; Mr. J. S. Sawyer made a contribution on 'The dynamical systems of the lower stratosphere'; Dr. F. J. Scrase spoke on 'Radio-sonde and radar wind measurements in the stratosphere over the British Isles'; and finally, Dr. G. M. B. Dobson who summed up the work in this field.

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Some recent work on ozone

By SIR CHARLES NORMAND

This contribution to the discussion bears on the question: to what extent is ozone an index of motion in the stratosphere? A few of the recent papers on ozone (*e.g.*, those by Dütsch 1946 and Nicolet 1945) have been partly or (*e.g.*, that by Reed 1950) wholly concerned to explain the daily changes in ozone amount as a result of horizontal and vertical air motion in the lower stratosphere.

For many years advection in the stratosphere has been regarded as, at least, a partial cause of the ozone changes. The reasoning is as simple as that which leads us to expect a south wind in this country to be warm and a north wind cold. For it is known that most of the ozone is in the stratosphere; the world survey organized by Dobson showed that the ozone amounts increase from the equator to the arctic and Meetham found that the correlation coefficients between total ozone and air temperature tended to increase up to the greatest heights (18 km) for which temperature data were available.

The effect of a vertical component of air flow must depend upon both the vertical distribution and the stability of ozone. The theoretical studies of formation and dissociation of ozone by Wulf and Denning, and the later ones by Nicolet and Dütsch, have paid particular attention to the stability or 'life' of ozone. From these studies the main point to note here is that at the higher levels, above 35 km, photo-chemical changes are rapid and quickly bring the ozone concentration whenever disturbed back to true equilibrium. At lower levels the air is more screened from the photo-active rays, the temperature is low and the rate of restoration of ozone to true equilibrium, when disturbed from it, is slow. At levels above 35 km, the time for half restoration may be of the order of hours or less, but below 20 km may be weeks or more. In the lower stratosphere, therefore, the ozone mixing ratio is a conservative property of a parcel of air and is determined mainly by the past history of that parcel of air, but in the higher stratosphere it is not conservative, being determined mainly by its location.

With regard to vertical distribution, ozone (unlike water vapour and the air itself) increases in concentration from the lower levels upwards, reaching a maximum in the stratosphere. Twenty years ago Götz, Dobson and Meetham deduced the average height or centre of gravity of the ozone to be about 22 km at Arosa, lowering slightly as the ozone amount increased. The maximum concentration or amount per km was estimated to lie at about 27 km and the maximum of the ozone-air mixing ratio higher still at perhaps 35 km on account of the rapid decrease of air density with height. More recently Ramanathan and Karandikar (1949) have reported ozone centres of gravity in India at about 26 km at Delhi and 28 km at Poona. From their Delhi observations they support the suggestion of Götz that there is a tendency for a second lower maximum in ozone concentration to

velop on days of higher ozone amount. Tönsberg and Langlo (1944) reached a similar nclusion from their observations at Tromso ; and from a preliminary examination the data from a V2 rocket flight over New Mexico in October 1947 Durand and colleagues duced two marked maxima at 25 and 17 km. Quite recently V. Regener has discussed sults from four balloon ascents in New Mexico in the spring of 1950, of which two show tendency to a double maximum. Now all these secondary maxima, except perhaps at of the V2 rocket which was a provisional estimate, no longer exist as maxima but come changes of slope on the curve, when the ozone is expressed as a mixing ratio stead of as a concentration or density. In general the maximum mixing ratio appears be well above 30 km, near the main source of ozone at the more photo-active levels. he tendency to the secondary maximum in concentration at or below 20 km is in line ith the earlier conclusion that when the total amount of ozone over a station increases e change takes place chiefly in the lower stratosphere, and there is every reason to expect at the vertical distribution of ozone, if we knew it from day to day over a large area, ould tell us much about the vertical component of the wind in the lower stratosphere. long an isobar in the lower stratosphere the ozone mixing ratio should increase in sub- ding air and decrease in ascending air. If it happens that north winds are generally sociated with subsidence and south winds with ascent of air in the lower stratosphere, en advection and vertical motion work together to cause an increase in ozone ; the gearing favourable. On the other hand when the gearing of the two motions is unfavourable eir effects on ozone will tend to cancel one another.

Reed estimated the results of an example of favourable gearing, postulating initial nditions of isobars, isotherms and isopleths of ozone parallel to latitudes and their sub- quent disturbance by an idealized depression. He concluded that the changes would l be in the direction to fit in with Meetham's correlations and Dobson's charts of ozone lues in pressure systems, but that the vertical motion would generate no more than a ird of the change normally observed, leaving therefore some two thirds or more of the ange to be explained by advection. In assessing this conclusion, let us not forget that ur knowledge about the detail of the vertical distribution of ozone is still insufficient and, out its variations in time and space, meagre.

Now leaving speculation aside, it is well to remind ourselves of some of the actual cts of the ozone-weather relationship. Since the 1934 paper by Meetham and Dobson the *Problems of meteorology* series, the fullest and best account, that by Dobson (1942), as not yet unfortunately become widely available in its full form but was summarised the 1946 Bakerian Lecture to the Royal Society. Accounts of similar, though more stricted, studies in Norway have been given by Tönsberg and Langlo as well as by Johan- n. The illustrations used below are selected from still more recent observations. New one spectrographs were established about 300 mi apart at Downham Market and Cam- rne in Nov. 1950 and observations have also been made daily at Oxford, about midway tween them. In Fig. 1 are shown the first six weeks of these daily observations along th the simultaneous height of tropopause at Oxford (H_c), the thickness (Th_{400}) of the 0–300 mb layer above Oxford which is a measure of the mean temperature in that layer, d the air temperature at the 150 mb level in the lower stratosphere. The H_c and Th_{400} rves have been drawn with inverted scale, as they are negatively correlated with ozone. will be noted that the three ozone curves show differences in detail but agree in their neral tendencies and in their major trends. The parallelism between these curves d the H_c and Th_{400} curves is clearly significant. The parallelism between ozone and the $_{50}$ curve is not so close, but still noticeable. Advection may account for much of the rallelism, because the main spells of northerly winds, as shown on the 300 mb weather art, occurred in the periods 21 to 25 Nov., 2 to 6 Dec. and 12 to 16 Dec. The second agram (Fig. 2) exhibits similar data for a later period, beginning 1 Jan. 1951. After e fall shown on all curves from 2 to 3 January, the parallelism between the ozone curves d the other curves breaks down and only becomes significant again after 25 Jan. On Jan. the ozone shows a marked opposition to the other curves, and a satisfactory planation is lacking for that and some other discrepancies.

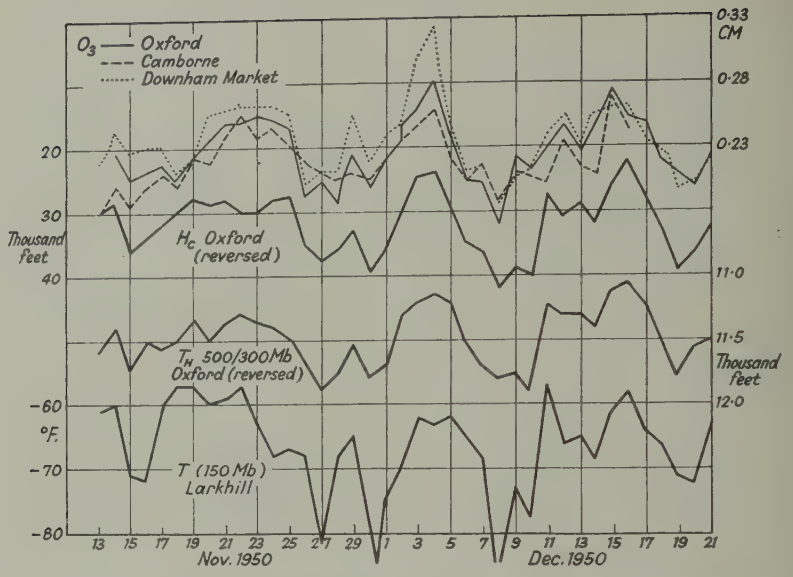

Figure 1. Daily ozone variations compared with simultaneous changes in height of tropopause, thickness of the 500-300 mb layer and temperature at 150 mb, 13 Nov. to 21 Dec. 1950. A period of high correlation.

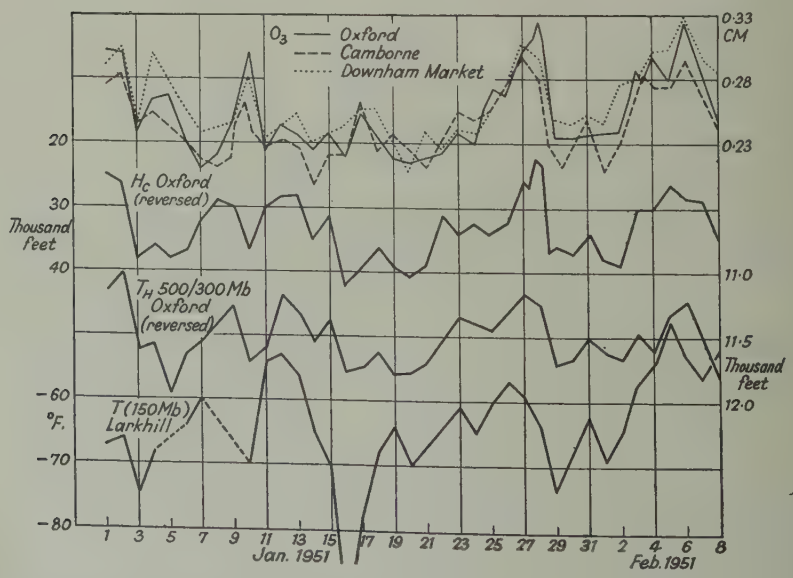

Figure 2. Daily ozone variations as in Fig. 1, but for the period 1 Jan. to 6 Feb. 1951. For much of this period the correlation was small.

TABLE I. CORRELATION COEFFICIENTS WITH OZONE, EXPRESSED AS PERCENTAGES

	Dobson, *etc.* in 1927	Meetham in 1937	OXFORD 13 Nov. to 8 Dec. 1950	9 Dec. to 3 Jan. 1951	4 Jan. to 29 Jan. 1951	30 Jan. to 24 Feb. 1951
face Pressure	− 46	..	− 46	− 69	− 32	− 46
ickness 1000/700 mb	(− 67)	..	− 81	− 47	− 44	− 56
ickness 700/500 mb	(− 67)	..	− 72	− 65	− 32	− 38
ickness 500/300 mb	(− 64)	..	− 89	− 71	− 27	− 67
mperature 250 mb	..	..	+ 33	− 09	+ 09	+ 50
opopause Height H_c	− 68	− 56	− 94	− 65	− 34	− 78
opopause Temp. T_c	..	..	+ 68	+ 35	+ 14	+ 38
opopause Pressure P_c	+ 57	..	+ 74	+ 53	+ 41	+ 77
mperature 200 mb	..	(+ 60)	+ 53	+ 40	+ 08	+ 67
mperature 150 mb	..	..	+ 62	+ 28	+ 17	+ 60
mperature 100 mb	..	(+ 63)	+ 60	+ 52	+ 53	+ 59
mperature 70 mb	..	(+ 70)	+ 59	+ 55	+ 53	+ 53

The bracketed figures in cols. 1 and 2 relate to geometric levels, the near equivalents of the pressure levels given in this table.
The data for 30 Jan. to 24 Feb. have been added since the discussion.
All the correlation coefficients of the final four columns are approximate, having been computed by Walker's short method.

The correlation coefficients for successive periods of 26 days each are given in Table I d, for comparison, some correlation coefficients calculated by Dobson *etc.*, in 1927 and eetham in 1937 are added. The correlations though varying somewhat from period period, are consistent in sign ; the correlations with temperature are all positive in the posphere and negative in the stratosphere, the only correlations with variable sign ng those with temperature at the 250 mb level, which is sometimes in and at other es above the troposphere. The newer correlations confirm the older results and are arly significant but obviously leave much to be explained.

Explanations of ozone changes at a station should, of course, be based on the actual ce distribution of the ozone at the time of occurrence and not on the normal distribution latitude and height. To gain routine estimates of the height distribution of ozone day day is still a considerable task, about which Dr. Dobson will be speaking later. To ermine the total amounts of ozone regularly at a network of stations is the more immediate d practical task, now in hand. The International Meteorological Association of the G.G.I., the Gassiot Committee of the Royal Society, U.N.E.S.C.O. and several national vices are taking a practical interest and bearing a financial share in an effort to set a network of ozone stations in Western Europe. It is hoped that there will be one trument each in Iceland, the Azores, Spitsbergen, Norway, Sweden, Denmark, Belgium, itzerland, France, Germany, Spain, N. Ireland and the Shetlands, in addition to three England. The instrument is the standard Dobson spectrophotometer with a photo-ltiplier replacing the photocell that was used in the pre-war design.

Elsewhere in the world the distribution of standard ozone instruments is (or is expected n to be) five of the new or modified type in the U.S.A., one each in Canada, Alaska, kistan and New Zealand and three in India.

To conclude, though ozone changes may often be due to advection and vertical motion he atmosphere, it is by no means proved that that is the whole of the story. The results he ozone survey over Europe in combination with those of the present network of upper d and temperature observations will provide valuable data for a more efficient examina-n of the causes of the ozone-weather relationships.

ACKNOWLEDGMENT

The ozone data of Camborne and Downham Market in Figs. 1 and 2 are reproduced h the permission of the Director of the Meteorological Office.

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551.571.7

Recent work of the Meteorological Research Flight of the Meteorological Office

By R. FRITH

The ceiling of the aircraft employed by the Meteorological Research Flight is about 40,000 ft and the stratosphere extends on the average from about 36,000 ft to about 100,000 ft. The M.R.F. are, so to speak, ground floor visitors, and so my remarks this evening can refer only to conditions at and near the tropopause.

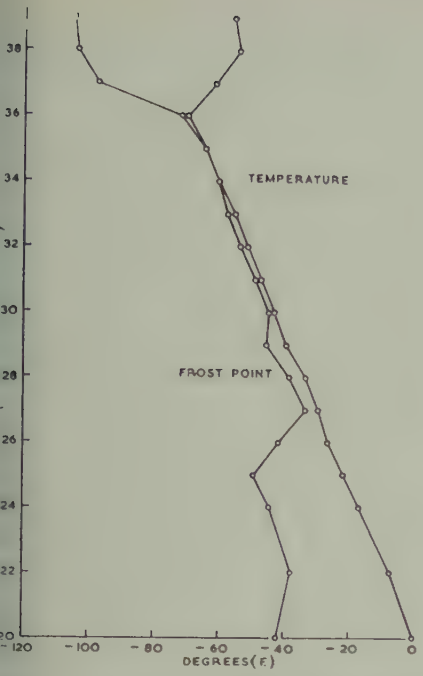
I shall confine my attention to humidity, with a passing reference now and then to temperature ; and I would like to say at once that most of the work I shall speak of is due to Mr. H. C. Shellard and Mr. J. K. Bannon, and is quoted with their kind permission.

Fig. 1 does not show a typical ascent. I have chosen it carefully as a straightforward example of a sharp fall of frost point at the tropopause. This sharp fall of frost point does not always occur. I cannot say just how often it does occur because before I could do that, someone would have to define what is meant by a ' sharp fall of frost point.' But to say that half the ascents show it – and half do not – gives the right idea.

There is a rather weak correlation between this sharp fall of frost point and the 'strength' of the tropopause inversion. In fact, when the tropopause is sharp, as here, then the frost point fall tends to be well marked. When the tropopause is not sharp, the frost point fall tends to be weak or non-existent.

In this respect the tropopause inversion behaves very much as the familiar subsidence inversion ; for in these also a sharp temperature inversion is generally accompanied by a sharp fall of frost point.

There is one peculiar feature of conditions in the tropopause however which seems to have no counterpart in subsidence inversions, namely, that sometimes the sharp fall of frost point occurs at a level several thousand feet away from the temperature tropopause (Fig. 2). It is difficult to visualize how this can come about, but it may be significant that there is a connection between its occurrence and the synoptic situation. In fact if the circulation at these high levels is cyclonic, then the sharp fall of frost point, if it occurs at all, is almost certain to coincide with the tropopause. On the other hand, if the circulation is anticyclonic then the sharp fall of frost point, if it occurs at all, is quite likely to be two or three thousand feet below the tropopause. It might be mentioned here that the sharp fall of frost point is much more likely to occur when the circulation is cyclonic than when it is anticyclonic.



gure 1. 14 Aug. 1950 – sharp fall in frost point at tropopause.

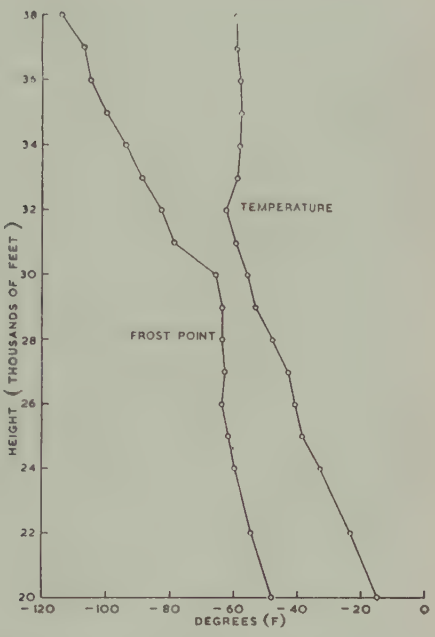


Figure 2. 2 October 1950 – sharp fall in frost point well below tropopause.

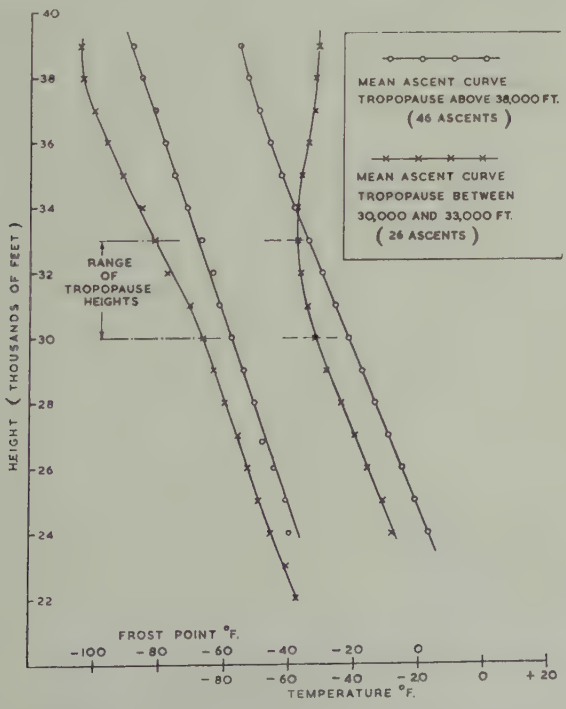


Figure 3. Mean ascent curves.

When we come to consider the behaviour of the frost point well inside the stratosphere we are severely handicapped by the fact that it is only when the tropopause is lower than usual that the ascent gets ' well inside the stratosphere.' Fig. 3 shows the mean ascent curve for ascents in which the tropopause lay between 30,000 and 33,000 ft, together with the mean ascent curve for ascents in which the tropopause was at or above 39,000 ft. We may therefore compare the behaviour of the frost point in the levels 34,000 to 39,000 ft, when these levels are in (a) the stratosphere (b) the troposphere.

We see that below the tropopause the lapse rate of frost point is constant at about 3·5°F/1,000 ft ; that above the tropopause, at any rate when the tropopause is low, the lapse rate is at first constant at about 5°F/1,000 ft, but that when the mean frost point has fallen to below — 100°F the lapse rate becomes rapidly less and it rather looks as though the frost point may soon become constant, at about — 110°F. There are, in fact, theoretical reasons for believing that this should happen, and on an American ascent, using an automatic frost-point hygrometer on a balloon, the frost point was found to become roughly constant at about — 94°F. (Barrett, Herndon and Carter 1949).

I would like to spend the remaining few minutes on two features of the frost point at high levels which are not necessarily connected with the stratosphere. First there is a remarkably high correlation between the frost point at high levels and the mean temperature of the troposphere. Following Priestley (1944) and taking the temperature at 500 mb as representative of the mean temperature of the troposphere, we find a correlation coefficient of 0·82 (127 cases) between the frost point at 250 mb and the temperature at 500 mb. Or, looked at in another way, knowing the temperature at 500 mb we can forecast the frost point at 250 mb and our forecast will be correct to within 5°F, on more than 60 per cent of occasions. It is worth while noting that 5° is not very much above the accuracy of our observations of frost point at these high levels.

Second, for a given tropopause height the air at high levels is relatively warm and dry when the upper circulation is cyclonic ; and relatively cold and moist when the upper circulation is anticyclonic. These facts are consistent with a scheme of a vertical motion in the upper air in which there is in anticyclones a general ascent of air above some level in the upper troposphere ; and general subsidence in cyclones above a similar level – a scheme suggested by Dr. Goldie (1949).

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551.510.53 : 551.515. : 551.558

The dynamical systems of the lower stratosphere

By J. S. SAWYER

In order to discuss problems of radiation balance (Goody 1949) or water vapour distribution (Brewer 1949) in the stratosphere, it has often been necessary to ignore the vertical and horizontal movements within the stratosphere which accompany the large- and small-scale weather systems of the troposphere. Although it may not be easy to develop similar treatments of the properties of the stratosphere when day-to-day fluctuations in in the air flow are considered, it is nevertheless desirable to review the available knowledge of the synoptic systems of the stratosphere as a basis for consideration of their significance in determining the average conditions at those levels.

The work of Prof. Rossby and others (Staff members of the University of Chicago 1947) has made us familiar with the fact that the large-scale zonal flow around the hemisphere

usually distorted into a sinusoidal stream the oscillations of which form the ' long-wave ' stems with a wave length of 50° to 120° longitude and amplitude of 5° to 30° latitude. ewed on the hemispherical scale the flow pattern has a similar shape in the middle tropo- here (500 mb), near the tropopause (300 and 200 mb) and in the lower stratosphere 00 mb). The wind velocity normally increases with height up to the tropopause and creases again upward into the stratosphere, but the direction remains much the same. he contours of the tropopause closely follow the long-wave pattern ; this association ntributes greatly to the well-known high positive correlation between tropopause height d the height of the 500 mb surface.

The flow within the long-wave pattern cannot be regarded as purely horizontal. If e motion within the stratosphere is regarded as approximately adiabatic, it is clear from e presence of warm air in the troughs and cold air in the ridges that in the motion of e air immediately above the tropopause descending motion predominates between a dge and the next trough downstream and ascending motion between trough and ridge.

With regard to dynamical developments on a smaller scale, it has been pointed out Sutcliffe (1938) that, because the horizontal thermal gradients reverse at the tropopause, e should expect that the sign of the relative divergence would be different for two layers, ie lying in the troposphere and the other in the stratosphere. We should therefore pect upward motion in the troposphere to be accompanied by downward motion in the ratosphere and *vice versa*. It is unlikely that the compensation is so exact as to give zero otion at the tropopause, but there is evidence for the existence of such complementary rtical circulations. Striking examples occur with vigorously developing depressions hich move slowly. In these, subsidence leads to a rapid warming of the lower stratosphere the same time that upward motion takes place in the troposphere (although possibly not exactly the same vertical column).

In a recent investigation at the Meteorological Office, Dunstable, consideration was ven to the vertical motion of the tropopause. It was found that following the air motion e potential temperature at the tropopause was conserved, within the limits of observation, er periods of up to 24 hr. This was taken to indicate that the tropopause normally oved with the air. It was further found that the average vertical displacement of the moving particle at the tropopause was about 1,000 ft in 6 hr (this includes motion in association th the smaller synoptic systems as well as with the long-wave pattern).

As a result of the nearly isothermal conditions within the stratosphere, vertical motion much more important in causing changes of temperature than it is within the troposphere. onsequently, on charts of the 200 mb level, where this is within the stratosphere, we find at vertical motion is at least as important as advection in controlling the temperature ld. For levels within the troposphere the synoptic meteorologist finds that areas of cold warm air move broadly with the wind ; within the stratosphere on the other hand such eas of warm or cold air are relatively slow moving and tend to retain a constant position relation to the troughs and ridges of the flow as the wind blows through them. How far ward into the stratosphere these vertical movements continue is not at present known.

On the synoptic charts of the high troposphere and the lower stratosphere, 300 and 0 mb, the passage of some depressions creates only a rather small disturbance of the neral flow – although numerically the changes in wind may be as great as near the ground, ey do not show up prominently against the background of the generally strong flow. her synoptic systems introduce major changes in the flow in the lower stratosphere.

However on any series of charts for high levels there are two types of synoptic feature hich occur regularly and which introduce large irregularities into the fields of both tem- rature and wind. These are the ' jet stream,' a conception introduced by the Chicago teorologists, and the ' tropopause funnel ' a concept introduced by Palmén. Both of ese features persist from chart to chart and show recognizably consistent features of ucture. Although often coupled with synoptic features within the troposphere, they occur independently and might therefore prove to be useful synoptic models for the alysis of high-level charts in the same way that the depression helps as a model in surface alysis.

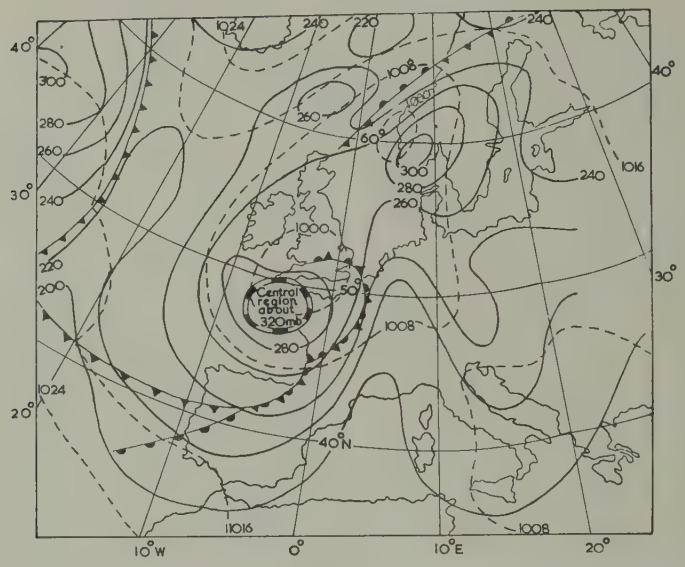

Figure 1. Isobars on the tropopause 0300 GMT 17 May 1949 (surface-pressure field shown by dashed lines).

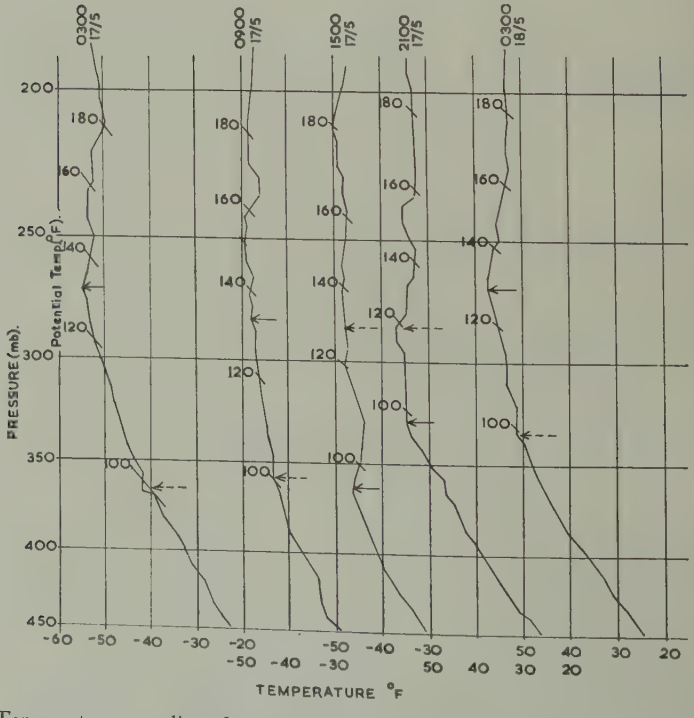

Figure 2. Temperature soundings from Larkhill 0300 GMT 17 May 1949 to 0300 GMT 18 May 1949

←— Tropopause ←— — — Other discontinuity in lapse rate

Short sloping lines indicate segments of dry adiabatics at potential temperatures shown.

The main features of a jet stream are sufficiently familiar from the cross-sections blished by Palmén and Nagler (1948). It consists of a narrow belt of strong winds in e upper troposphere decreasing rapidly in strength both horizontally and vertically. ithin the stratosphere the most significant features are the rapid upward decrease of nd above the jet and the corresponding strong temperature gradient from the belt of arm air to the north of the jet to the colder air to the south. The decrease of wind may as much as 10 kt/1,000 ft over a range of 5,000 ft which requires a horizontal temperature adient exceeding 10°F in 100 mi. Moreover the jet stream is nearly always associated th a discontinuity of the tropopause ; the tropopause on the south side of the jet may higher by 5,000 ft than that on the north side. The discontinuity is not necessarily the me all along the jet, and there is some evidence that it becomes greater towards the exit – deed it is probable that the discontinuity is the result of a folding-over taking place at e entrance to the jet.

Another important feature of charts of the lower stratosphere is the warm areas sociated with the ' tropopause funnels ' of Palmén. In these there is a centre of low tropo- use, warm stratospheric air and a cyclonic circulation ; both wind and temperature stribution may be approximately symmetrical about the centre but are often more irregular. the central area the tropopause appears to be substantially lower and at a lower potential mperature than in the periphery, and there is probably a circular discontinuity of the opopause around the centre. Fig. 1 shows the contours of the tropopause around such ' funnel ' by means of isobars on the tropopause, and Fig. 2 shows a series of radiosonde cents as the centre of the funnel passed over Larkhill. These tropopause funnels seem form rapidly in troughs in the upper flow pattern in association with the vigorous deepen- g of a rather slow-moving depression. Once formed the tropopause funnel seems to a stable dynamical and thermal system. It is often, but not always, associated with a ld pool in the troposphere.

This brief review of some of the dynamical features of the lower stratosphere is by no eans exhaustive, but it is offered as a guide to those studying other properties of the lower ratosphere.

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551.510.53 : 551.524.7 : 551.557.3

Radiosonde and radar wind measurements in the stratosphere over the British Isles

By F. J. SCRASE

1. Introduction

The successful development of a British 2 kg sounding balloon (Ashford and Harrison 947) has enabled the Meteorological Office to explore the stratosphere by means of radio- nde and radar wind equipment up to heights of about 100,000 ft (30 km). The results the first few ascents in the summer of 1949 were described by Scrase (1949). Since then

a more extensive series of these high-altitude ascents has been in progress, mainly with a view to covering all seasons of a year at two widely separated stations in the British Isles. The series is not quite completed but sufficient observations are available to enable a preliminary account of the main results to be given.

2. The ascents

The Meteorological Office radiosonde normally measures pressure, temperature and humidity, each element being switched into the transmitting circuit in turn by a wind-driven switch. Since this cannot be relied upon to continue in operation at very low air densities, the instruments used for the high-altitude observations were modified to indicate temperature only. In order to avoid errors due to solar radiation the soundings were made at 22 GMT. For the measurement of wind and height up to the high levels the range of the radar equipment was extended to nearly 50 mi ; this enabled the measurements to be made up to 100,000 ft provided the mean vector wind over the whole height between the ground and that level did not exceed 33 kt (the maximum winds measured in some of the soundings were more than three times this velocity).

Most of the soundings were made at Downham Market (52°36′N., 00°24′E.) and Lerwick (60°08′N., 1°11′W.). Of a total of 66 successful ascents 81 per cent reached 80,000 ft and 30 per cent reached 100,000 ft ; the highest was 110,000 ft. The results have been separated into two groups representing the winter and summer half years, *viz.*, October to March and April to September inclusive. Graphs have been plotted showing the mean values of temperature and wind speed at intervals of 5,000 ft. Since the number of ascents differed greatly from one month to another, equal weight has been given to each month in deriving these means. For the winter values for Lerwick, however, no observations are yet available for November and December.

3. Winter observations

In Fig. 1 the curves for the winter half-year show a slight, but continuous, decrease of temperature with height above the tropopause, the lapse rate being about 0·2°F /1,000 ft.

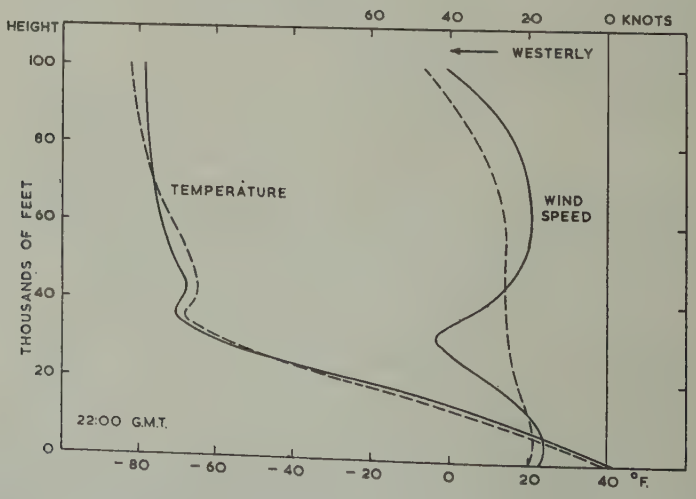

Figure 1. High-altitude soundings : means for winter half-year (Oct.-Mar.). Full line = Downham Market (20 ascents) : broken line = Lerwick (9 ascents).

e Lerwick temperatures were about 4°F higher than the Downham Market temperatures the lower stratosphere and about 4°F lower at 100,000 ft. At this height the average nperature in the winter half-year was about — 80°F ; individual observations varied up about 20°F on each side of this.

In the winter half-year winds with a westerly component persisted at all levels in the atosphere. From a minimum at 60,000 ft the wind speeds increased with height up to highest levels reached. Average speeds at 100,000 ft exceeded 40 kt.

4. Summer observations

For the summer half-year, the results, which are given in Fig. 2, showed the isothermal ion of the stratosphere extending from just above the tropopause to 80,000 ft. Above t the temperature rose at about 1°F/ 1,000 ft at Downham Market and at 0·6°F/ 1,000 ft Lerwick. In the lower stratosphere Lerwick temperatures were about 6°F higher than se at Downham Market and the same amount lower at 100,000 ft. At this height tem- atures averaged about — 46°F ; the scatter of individual observations was up to about F on each side of this value.

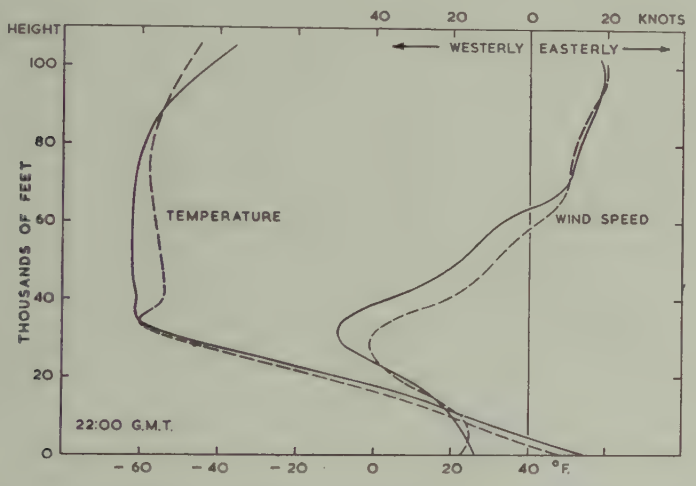

Figure 2. High-altitude soundings : means for summer half-year (Apr.-Sept.). Full line = Downham Market (20 ascents) : broken line = Lerwick (17 ascents).

The variation of wind with height was much the same at both stations. After reaching maximum near the tropopause the speed decreased with height in the lower stratosphere a minimum at 60,000 ft and then increased relatively slowly up to the highest levels ched. At 60,000 ft the north-westerly winds of the lower levels gave way to the easterly rent which extended up to the greatest heights observed. The results therefore confirm ipple's (1935) suggestion of a monsoon wind at high levels, and the observations of rgatroyd and Clews (1949).

5. Monsoon winds in the stratosphere

As is now well known the changes from westerly to easterly winds, and *vice versa*, in stratosphere occur near the equinoxes. In Fig. 3 the heights of the boundary between lower westerly stream and the upper easterly current, when it existed, have been plotted inst the time of year. The graph shows a fairly sudden onset of the easterly current at higher levels at the vernal equinox and a cessation two or three weeks before the autumnal inox. It also shows a fairly solid penetration of the easterly current down to about

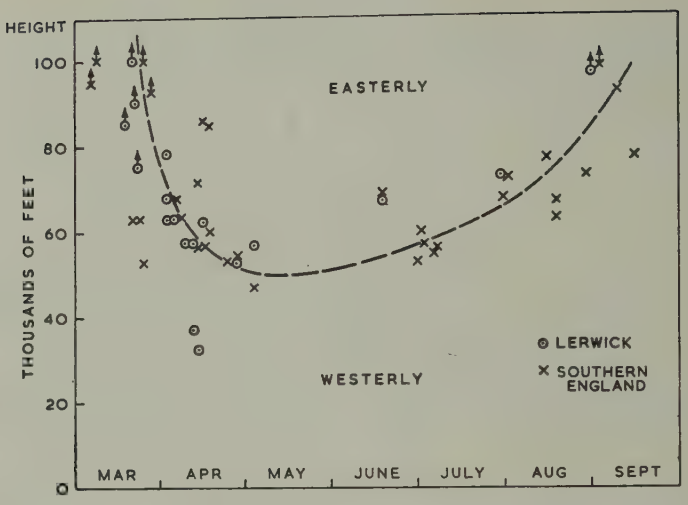

Figure 3. Boundary of winds with easterly and westerly components (there were no easterly components below 100,000 ft from Oct.-Feb.).

60,000 ft in spring, but after mid-summer the height of the boundary increased gradually as the autumnal equinox approached. The change in the wind regime appeared to be more clearly defined at Lerwick than at Downham Market. At the latter station, after the first onset of easterly winds in the middle of March, temporary reversions to westerly type occurred at the end of that month and in mid-April ; similarly after a change back to westerly winds in early September there was a temporary reversion to easterly winds in the middle of that month. Gutenberg (1949) has reported that in New Mexico a change from westerly winds (below) to easterly winds (above) in the lower stratosphere occasionally occurred during the winter months. There was no indication of this at Lerwick or Downham Market.

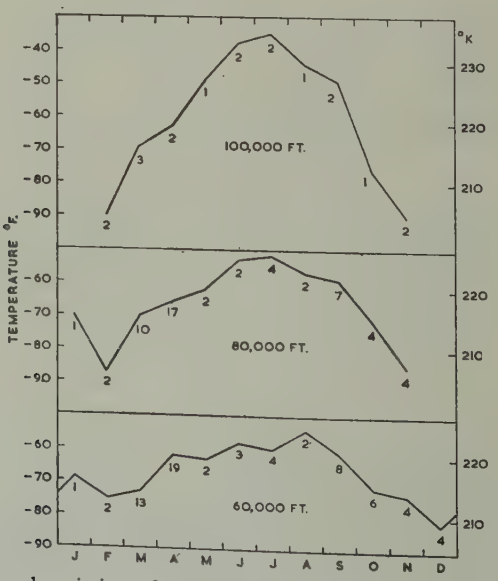

Figure 4. Annual variation of temperature at high levels over the British Isles.

6. Annual variation of temperature at high levels

Sufficient observations have been obtained to indicate the trend of temperature varia- n at high levels through most of the year. The results for three levels from 60,000 to),000 ft are given in Fig. 4, in which the observations for the two stations are combined, figures against the curves being the number of observations so far available. The data 60,000 ft are reasonably consistent with long-period averages obtained from routine iosonde observations. At this height the maximum temperature occurred in August, ich is to be expected if the absorption of long-wave out-going radiation is the controlling tor. At higher levels, however, the maximum was much nearer to the summer solstice. is type of variation has been explained by Dobson, Brewer and Cwilong (1946) as being e to absorption of solar radiation by ozone, which occurs in greater quantities during the ing and would therefore tend to produce a maximum effect before, rather than after, summer solstice.

7. Comparisons with other observations

Comparison of the average winter and summer temperatures at 100,000 ft with data ently published by Brasefield (1950) and Gutenberg (1949), using radiosonde methods, l by Crary (1950), using acoustical methods, shows the following interesting variation h latitude.

TABLE I

thor	Place	Latitude	Temperature at 100,000 ft Winter	Summer
ary	Alaska	65°N.	− 91°F	− 62°F
ase	Lerwick	60°N.	− 82	− 48
ase	Downham Market	52°N.	− 78	− 42
sefield	New Jersey	40°N.	− 45	− 37
tenberg	New Mexico	32°N.	− 56	− 40
ary	Bermuda	32°N.	—	− 42
ary	Canal Zone	9°N.	—	− 65

The well defined maximum of temperature in middle latitudes is unexpected in view the fact that the total amount of ozone increases continuously with latitude. Possibly re is a marked variation in the vertical distribution of ozone with latitude such that the ntent at 100,000 ft has a maximum in middle latitudes.

The summer observations of wind at all the stations referred to above showed the rease in the easterly wind with height in the upper stratosphere. The increase in the sterly wind at high levels observed at the British stations in winter was also evident in Alaska and New Jersey observations, but Gutenberg reported that a decrease usually vailed in New Mexico.

When the present series of soundings is completed more detailed comparisons with er data and with synoptic conditions will be studied and it is hoped also to provide ne information on the diurnal variation of temperature at 100,000 ft.

Acknowledgment

I am indebted to the staffs of the Lerwick and Downham Market radiosonde stations undertaking the special high-altitude ascents in addition to their routine soundings l to the Director of the Meteorological Office for permission to publish the liminary results.

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Recent work on the stratosphere

By G. M. B. DOBSON

1.

I think that discussions such as today's serve a very useful purpose in bringing together the results of various workers on some particular part of meteorology. Not only is the discussion stimulating to those present, but the published record provides a very valuable reference for a much larger number of meteorologists. These discussions are particularly opportune at a time when there have been notable increases in our knowledge of the subject, and this is certainly the case with our subject today. This increase in our knowledge of the stratosphere has a number of causes and I would put the chief of them in order of importance as follows :

(1) The wide network of stations where radiosondes are now sent up once, twice or four times every day. It was only in 1941 that I found that I could make no progress in a particular problem concerning the upper atmosphere because there were no adequate upper data. Today the Upper Air Section of our *Daily Weather Report* gives us an amount of detail which, if still not all we could desire, is almost as much as one meteorologist can digest daily ! We are even begining to know something about the climatology of the upper air over a large part of the world.

(2) Second only to radiosondes in importance, I would put the existence of efficient Meteorological Research Flights suitably equipped with the necessary aircraft. In this country we are fortunate in having such a Research Flight which, though small in number of personnel is, I believe, unsurpassed by any in the world. Our knowledge of the humidity of the upper troposphere and lower stratosphere is entirely due to the British Meteorological Research Flight and this is only one of the many subjects about which they have obtained valuable and entirely new information.

(3) The work of the late Dr. F. J. W. Whipple on air waves (Whipple 1931, 1932, 1934) first gave us a fairly accurate picture of the temperature in the atmosphere up to heights of 50 km and recently our friends across the Atlantic, with characteristic boldness, have carried out a series of measurements on air waves by using aircraft to drop bombs at varying distances along lines radiating out from the recording station. In this way they have been able to make allowance for the wind in the upper atmosphere which gave Dr. Whipple so much trouble. Their results at two widely different latitudes confirm Whipple's work though they indicate rather lower temperatures at the greatest heights.

(4) The special high-altitude radiosondes which Dr. Scrase has just described might be included under the general heading of radiosondes but they really open up levels above

ose normally reached by routine balloon-sondes and may therefore merit special mention.

(5) Measurements of the amount of ozone present in the upper atmosphere are now being ade by a number of people and Sir Charles Normand has already described the network stations in and around NW. Europe where daily measurements of ozone are being should soon be made. That there is a close connection between the changes in the ozone d the meteorological conditions in the upper air is well known and likewise we know mething of the vertical distribution of this ozone from the work of Götz (1934) in itzerland, Tönsberg and Langlo (1944) in Norway, Karandikar and Ramanathan 949) in India using methods dependent on measurements made on the ground, while e Regeners in Germany (E. and V. Regener 1934) and later V. Regener (1950) in New exico have sent up spectrographs on balloons for this purpose.

(6) Rockets are only just coming into their own for upper-air exploration but already ost interesting results have been obtained at heights quite inaccessible to any other direct ethod and we hope for much more from them.

(7) Last, but by no means least important, we have had an increasing number of theor- cal people thinking about the problems of the chemistry and physics of the upper air. he interplay between their work and the observational results is of great importance.

2.

Perhaps I have already spent too much time on the past and I would now turn to nsider what are the chief problems which seem to lie immediately ahead and in which e may reasonably hope that progress may be made so that, when the time comes round r our Society to hold another discussion on the stratosphere, the speakers will then be le to describe their partial, if not complete, solution.

(1) First among these immediate problems I think I would place the general circulation er the whole world up to a height of 30 km or more. I put this first because it affects many other problems. It is only since the widespread radiosonde observations have en available that we have had any adequate data about this and it seems unlikely that we all have anything like a complete knowledge of the general circulation for a very long time, ut I think that we may hope for some substantial increase in knowledge. The work that s already been done by Dr. Rossby and his collaborators at Chicago is most encouraging d the number of observations of upper winds in widely different parts of the world is creasing while the general winds, particularly at high altitudes seem to be zonal with rge sinusoidal waves, it seems that the mixing of the air between latitudes takes place in e smaller, irregular circulations which we call cyclones and anticyclones. A slow circu- ion of the air – moving upwards near the equator, then moving polewards at great heights, d descending in medium or high latitudes and finally returning equatorwards at low vels, has already been suggested as the cause of both the latitudinal variation of atmos- eric ozone and of the very dry air found at levels around 12 km, both of which we shall scuss later.

(2) The second question on which I hope that we shall have much fuller knowledge thin the next ten years or so, is the radiative equilibrium of the upper air. It has usually en assumed up to the present time that heat is transferred through the troposphere iefly by turbulent diffusion and convection, while in the stratosphere radiation only is portant. There are now many reasons for thinking that this is much too simple a picture. Ve have no measurements to show whether or not the air in the stratosphere is actually radiative equilibrium, though it should be possible to make such measurements and they e very desirable. If the air is not in radiative equilibrium we shall want to know what the relative importance of the transfer of heat by radiation and by other methods. Clearly, most, we could only expect the air to be in radiative equilibrium *on the average* and henever it changes latitude, for example, it must warm or cool until it takes up its new uilibrium. We want to know how quickly it will take up this new equilibrium. Again, variations in the proportions of water vapour, carbon dioxide and ozone (the chief gases sorbing radiation in the upper air), cause appreciable changes in the temperature of

the air at great heights ? Or are the changes of temperature due to dynamic effects ? Most probably in the stratosphere, as in the troposphere, the temperature is determined by a balance of all the processes, but probably with different relative importance.

I doubt if any meteorologist could say what is the real physical significance of the tropopause. Formal rules have to be adopted for fixing the nominal position of the tropopause for statistical purposes, but in many cases such rules give a level for the tropopause which is almost certainly not that which physical principles would indicate. I hope we may look forward to being able to fix the tropopause (where a real tropopause exists) on purely physical principles.

(3) There are a number of problems connected with atmospheric ozone which should not be too difficult to solve :

(a) What is the vertical distribution of the ozone in different meteorological conditions, in different parts of the world and at different seasons ? This is of great importance in answering some of the next questions. Measurements by spectrographs carried on rockets or on large balloons cannot be made in great numbers and the only simple method of obtaining the vertical distribution which is suitable for semi-routine observations is that depending on measurements of the clear zenith skylight near sunrise or sunset. Fortunately

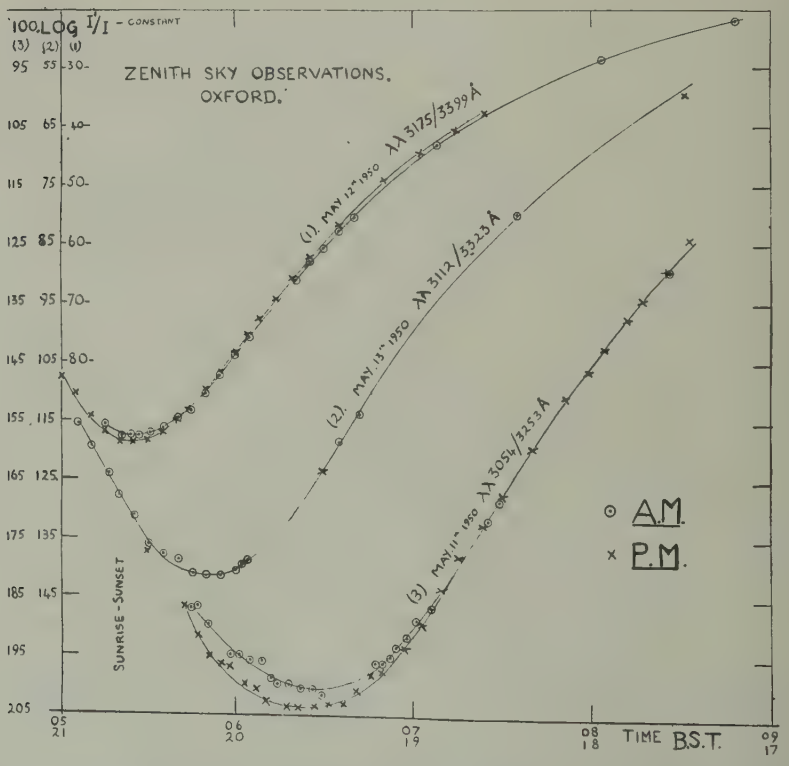

Figure 1. Measurements of zenith skylight near sunrise and sunset. The points give the measured ratio of the intensity of two adjacent wave lengths for three such pairs of wave lengths. The small scatter of the points from a smooth curve is an indication of the small random error of the observations. The shape of such curves depends on the vertical distribution of the ozone in the atmosphere. The fact that the curves reverse their slope near sunrise and sunset is due to the fact that at these times much of the light of the shorter wave lengths is scattered by the air at a great height which is above most of the ozone.

ch measurements can now be made with good accuracy (Fig. 1) but we still require much lp from our mathematical colleagues in the interpretation of the measurements before e can deduce the vertical distribution of the ozone with certainty.

(b) It is well known that there are large variations of ozone from day to day which show striking relation to the weather conditions as Sir Charles Normand has described. But, he has also shown, the simple relations sometimes entirely break down and we still do ot know at all what is the real, ultimate cause of these ozone variations or the reason for eir close connection with weather conditions. We believe that the ozone is mainly formed mewhere between about 25 and 35 km – this being about the height of the maximum one-air ratio by the action of solar ultra-violet radiation. The actual ozone found at any eight and any place may have come there either by vertical diffusion or by vertical diffusion nd advection. What is the relative importance of these two processes and at what height advection important? A knowledge of the vertical distribution mentioned under (a) ill greatly help.

(c) Why should there be the peculiar distribution of ozone with latitude and with season? We still do not know the seasonal variation of ozone in very high latitudes with certainty ut observations using moonlight (Fig. 2) should help very much). Here again a knowledge f the general circulation of the atmosphere over the whole world would be a great help. t has been suggested that the ozone is low near the equator because there are ascending urrents there which carry air, poor in ozone, up into the higher layers. The air on its way olewards acquires more ozone under the action of solar ultra-violet radiation and when

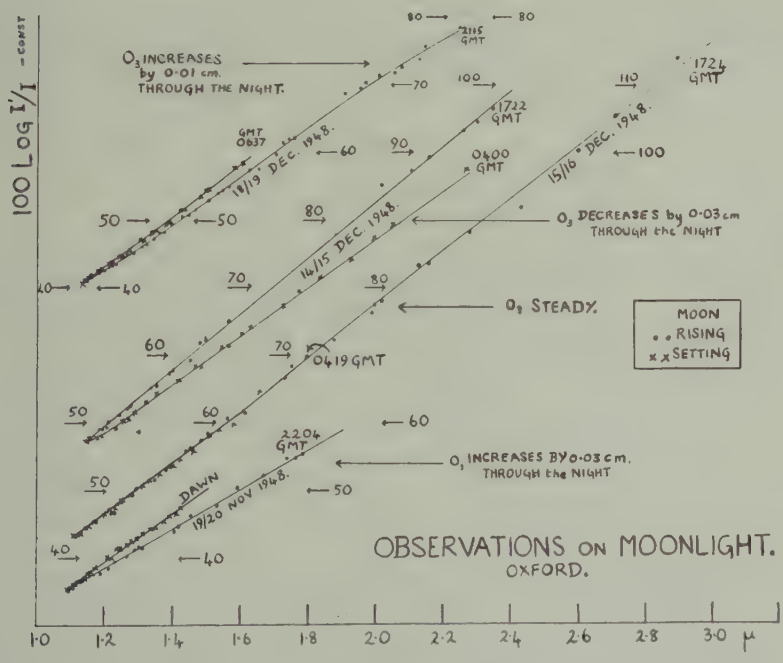

Figure 2. Observations using moonlight at Oxford.

rdinates are values of log I/I', abscissae are path lengths of the light through the ozone layer. If e ozone remained constant the points should lie on a straight line whose slope depends on the amount ozone. The difference in the slopes of some of the lines on the same night for moon-rising and oon-setting is due to a real change in the ozone through the night and was to be expected from the eteorological conditions. As in Figure 1, the small scatter of the points indicates a small random error in the observations.

it descends in higher latitudes it carries this ozone-rich air downwards so that a deep layer of ozone-rich air is produced and the total ozone content is high. Again we want to know the vertical distributions of the ozone.

(4) The humidity of the upper air. Measurements of the humidity of the air in the stratosphere are of great interest. While it is of negligible importance thermodynamically by reason of the very small amount present even in saturated air, yet it is an almost ideal ' tracer ' by which to identify air masses in the stratosphere, seeing that it must be extremely ' conservative.' In addition to its value as a ' tracer ' we have already referred to its importance in the thermal radiative equilibrium of the stratosphere.

Methods have now been developed of measuring the humidity of the air up to the greatest heights which can be reached by meteorological aircraft but we want information at still greater heights and, of course, at many places widely distributed over the world. Much is hoped from measurements of the infra-red absorption of solar radiation by apparatus carried on aircraft and the present frost-point method used on aircraft can be extended to automatic instruments carried on free balloons ; indeed some measurements have already been made in America (Barrett 1949). We also want to know much more about the variations of humidity at great heights with different meteorological conditions. Why the air is so very dry at great heights is still somewhat of a mystery. It may be that in the general world-wide circulation of the atmosphere, air is cooled as it rises through the equatorial stratosphere, the water vapour being condensed to ice crystals and the ice crystals then fall out, leaving the air saturated at the lowest temperature to which it has been submitted. Thereafter the air passes gradually to higher latitudes within the stratosphere and has no chance to pick up more water vapour. However the humidities which Dr. Frith has just described are so low that this explanation is only just possible. On the average the humidity is still falling with increasing height up to the ceiling of the aircraft and if, as seems possible or even probable, still lower humidities are eventually observed at say 15 km some other explanation will have to be found.

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Discussion

Mr. E. Gold : I should like to ask Dr. Scrase if the ascents to 100,000 ft at Lerwick in the middle of the summer reached sunlight, although released after sunset.

With regard to Dr. Dobson's remark about the tropopause, the rules for its determination introduced 30 years or more ago were probably the best that could be devised at that time to enable the broad features to be obtained. But there are clearly some cases where there is really no definite tropopause. The cases where there is a definite physical tropopause ought to be separately distinguished to receive separate statistical treatment.

Miss N. Carruthers : Is a ' tropopause funnel ' a physical entity ? The example shown by Mr. Sawyer, I noticed, was associated with a depression and I wondered whether the low tropopause heights recorded in the ' funnel ' were true tropopause heights or whether they had arisen from confusion with a front.

Mr. D. D. Clark : When showing the results of the observations on the zenith sky, of. Dobson reminded us that, although the instrumental part of these observations d been solved satisfactorily, there was, however, no mathematical treatment that would e the vertical distribution of ozone from the measurements.

Prof. Dobson remarked that the inverse problem could be tackled, namely, given an one distribution, to find the absorption curves. One would have thought, therefore, at some sort of answer could have been obtained by constructing large numbers of solu-ns of the inverse problem and then obtaining the distribution by matching curves.

Can Prof. Dobson say, please, how far the mathematicians have gone in this problem d what is the nature of the difficulties confronting them ?

Asst. Prof. P. A. Sheppard : We frequently read and have again been told tonight at ozone increases in amount from the equator to the poles and from autumn to spring. would ask Professor Dobson to tell us whether and in what respects these statements quire qualification. I am thinking particularly of course of high latitudes in which the arked seasonal variation in insolation could conceivably modify the above statements th profit to our ultimate understanding of ozone variation.

I was very struck with Dr. Scrase's slide (Fig. 1) showing the winter variation of zonal nd with height. It is quite new to me that a marked increase of westerly wind sets in at out 20 km, indicating that the reverse thermal wind of the lower stratosphere (increase temperature with latitude) gives place to the opposite regime (temperature decreasing th increase of latitude, as in the troposphere) in the middle stratosphere. Perhaps this ight have been expected from the fact that the temperature inversion of the low-latitude atosphere gives place to a lapse of temperature in the stratosphere of high latitudes, variation which has its ' reflection ' in Dr. Scrase's slide (Fig. 4) of the change from in-rsion to lapse from summer to winter in the stratosphere over the British Isles. But ain, this seasonal variation has not I think been brought out so clearly before tonight. is figures for the meridional temperature variation at 30 km are in line with the behaviour ferred to, but they show also that there is yet more complication to be taken account in the integration of stratosphere phenomena.

Replies to discussion

Dr. F. J. Scrase (*in reply to* Mr. E. Gold) : In none of the soundings at Lerwick did e radiosonde become exposed to sunshine at high levels. At the summer solstice the ne of sunset at Lerwick at 100,000 ft is 2314 GMT which is 5 min before a balloon released 2200 GMT would reach that height. In fact none of the summer soundings which ached 100,000 ft was made within two or three weeks of the solstice.

Dr. F. J. Scrase (*in reply to* Asst. Prof. Sheppard) : The increase in the speed of the esterly winds with height above 20 km in the winter half-year is certainly well marked d the observations of the individual ascents generally show this feature, though there e some exceptions. I have delayed making any computations of the thermal winds until few more observations at the high levels are available for the winter months. It will be rprising if the horizontal temperature gradients between Lerwick and Downham Market e inconsistent with the observed regimes.

Mr. J. S. Sawyer (*in reply to* Miss N. Carruthers) : The low tropopause in the centre a ' tropopause funnel ' often appears to be continuous with the adjacent cold front rface and to be separated by a discontinuity from other parts of the tropopause. However e low tropopause usually satisfies conventional definitions of the tropopause based on ose rate, and it is difficult to define any other ' true tropopause ' in the absence of a ysical basis for such a definition.

Dr. G. M. B. Dobson (*in reply to* Mr. D. D. Clark) : A very serious difficulty is the mplication of the problem due to the fact that sunlight is scattered by the air at all levels wn into the instrument. To make it possible to deal with the problem some simplifying sumptions have to be made and yet these must not depart appreciably from the actual nditions existing in the atmosphere. In those cases where the spectral composition of

the skylight has been accurately calculated the distributions of ozone which were assumed have been very far from what we know to be the actual distribution.

An even greater difficulty is probably that due to the fact that not only sunlight which has been once scattered by the air enters the instrument, but sunlight which has been scattered twice, three times and more also enters the instrument as well. The light is being absorbed by the ozone during each stage of its progress through the atmosphere.

Dr. G. M. B. Dobson (*in reply to* Asst. Prof. Sheppard): With regard to our present knowledge of the distribution of ozone over the world, I think that it would be true to say that we have a fairly good idea of the distribution over the world between the latitude of New Zealand in the south and the Shetland Isles in the north at all seasons. (This assumes that places of the same latitude have more or less the same ozone, and no cases to the contrary have yet been found). To the south of New Zealand our knowledge is very slight. In the north there are good observations at Abisko, and Tromsö in the spring, summer and autumn and in the summer and early autumn in Spitsbergen. Observations have also been made during the winter at these northern stations using the light from the moon or stars but their accuracy is naturally less good. It appears at the present time that there may be an area of low ozone in very high latitudes during the winter which persists until about mid-January when there is a rather sudden rise to quite high values.

During the present winter further observations are being made at Tromsö and Spitsbergen but the results are not yet available.

Report of the Council

for the Year 1950

(Submitted to the Annual General Meeting, 18 April 1951)

General

1950, the Centenary year of the Society, was, as might have been expected, particularly eventful year with a wide range of activities to record. The Centenary eetings held at Oxford and London were very well attended and attracted dele-tes from all over the world. A full account is given in the *Centenary Proceedings the Royal Meteorological Society*, 1950, a volume of nearly 300 pages. The Society's uilding was renovated and in some respects modernized as far as limited finances, ded by a Centenary Appeal to Fellows, would permit.

A completely revised and enlarged edition of Inwards's *Weather Lore* was ublished on behalf of the Society and also a set of 24 postcards illustrating cloud-pes from the Clarke Collection.

All these activities are reflected in the very satisfactory membership position given below :

Table A

	Fellows and Foreign Members			Asso-ciates	Student Asso-ciates	Hono-rary Mem-bers	TOTAL
	Annual	Life	Total				
1 Dec. 1949	1,262	196	1,458	122	61	13	1,654
ince admitted	+109	+ 3	+112	+104	+28	+5	+249
Readmitted	+ 5	—	+ 5	—	—	—	+ 5
Transferred	+ 3	+ 2	+ 5	—	— 5	—	—
Deceased	— 16	— 4	— 20	—	—	—	— 20
Resigned etc.	—145	—	—145	—	—	—	—145
1 Dec. 1950	1,218	197	1,415	226	84	18	1,743

The inflow of scientific communications, both for the *Quarterly Journal* and r the magazine *Weather*, has been well maintained. 55 papers were submitted r publication in the *Quarterly Journal* during 1950 as against 48 in 1949, and in ddition 28 papers were published in the *Centenary Proceedings of the Royal Meteoro-*

logical Society, 1950. The arrears of publication in the *Quarterly Journal* were cleared and with the exception of the April number (following the Centenary celebrations) all the issues in 1950 appeared in the month due for publication.

It is appropriate that the centenary year of the Society should have also marked the inception of vacation courses in elementary synoptic and observational meteorology. The pioneer course was held in September at the Malham Tarn Centre of the Council for Promotion of Field Studies, and was an outstanding success both as regards attendance and the calibre of the students. Further and extended courses are being arranged in 1951.

During the year a By-laws Committee under the chairmanship of Mr. J. F. Shipley undertook the task of revising the existing by-laws, so as to ensure the satisfactory representation of the Fellows in the constitution of the Council and in its election and also to simplify the administrative work of Council. The draft of the new by-laws was adopted at a Special General Meeting on 13 December and the new by-laws became effective from 1 January 1951. The Council wish to express their appreciation of the time so ungrudgingly given by Mr. J. F. Shipley and members of the By-laws Committee in carrying out this important work.

In presenting the details of the Society's activities under various headings, the Council feel confident not only that the Centenary year made a most valuable contribution to the Society's hundred-year old record but that the opening of the second century will be marked by an ever-growing expansion of the Society's work in the advancement of Meteorological Science.

Membership

The total membership of the Society set out in Table A shows the gratifying increase of nearly a hundred. It is a particularly good omen that the Society begins its second century with the largest membership on record.

The Council wish to pay a special tribute to the Canadian Branch, whose healthy and vigorous growth in 1950 was largely responsible for the increase in total membership. At the same time it is important to note the increase of 23 in Student membership (as compared with 4 in 1949 and 8 in 1948). Ten of the new Student members joined after attending the vacation course at Malham Tarn.

The Council record with regret the death of the following Fellows ; dates of election are appended to the names :

Dr. J. R. Ashworth (1934)
C. A. Bell (1944)
Major A. F. Bowker (Life Fellow, 1892)
H. C. Bowker (1946)
A. Hampton Brown (Life Fellow, 1936)
C. J. P. Cave (Life Fellow, 1899)
I. M. Crawford (1950)
Capt. R. D. Hog (1936)
Prof. E. A. Milne (1933)
A. G. Murdoch (1948)
Capt. H. S. Murray-Smith (1926)
C. Dick Peddie (1910)
J. Rees (1913)
G. P. Smith (1946)
Capt. H. A. Staples (1923)
A. Stocks (1919)

C. Marsh (1920)
H. R. Mill (Life Fellow 1893), President & Secretary, Symons Medallist 1918
H. N. Warren (1946)
A. Y. Worthington (1930)

Dr. James Reginald Ashworth, who died on 9 July 1950 in his ninetieth year, s well known as a specialist in magnetism and in the measurement of atmos-eric pollution. For nearly two-thirds of a century he had been honorary secretary the Rochdale Literary and Scientific Society.

Mr. Arthur Hampton Brown joined the staff of the Society in 1896 and held h distinction the post of assistant secretary from 1915 until his retirement on sion in 1935, when he received a presentation and was elected to honorary life mbership in tribute to the value of his services. Thereafter he gave important untary help with the Society's sub-editorial work almost to the time of his death the age of 75 on 25 November 1950.

Mr. Charles John Philip Cave was a pioneer in upper-air research and a cloud otographer of international repute. In 1913-15 and again in 1924-26 he served president of the Society. His numerous published contributions to meteoro-ical literature include many papers and notes in the *Quarterly Journal*. He died his eightieth year on 8 December 1950.

Dr. Hugh Robert Mill, a noted geographer, an authority of world-wide recog-ion on Polar exploration, and successor to George James Symons as head of the tish Rainfall Organization, was president of the Society in 1907-09 and Symons edallist in 1918. He died at the age of 89 on 5 April 1950, two days after the public nouncement of his having been selected for the first award of the Hugh Robert ll Medal and Prize newly instituted in his honour by the Society.

Professor Edward Arthur Milne, M.B.E., F.R.S., whose sudden death at the of 54 occurred in Dublin on 21 September 1950, was one of the most distin-shed mathematical physicists of his day. For more than a quarter of a century had been a leader in the progress of cosmology and astrophysics. He was awarded Royal Medal of the Royal Society in 1941 and was president of the Royal Astro-nical Society in 1943-45.

Mr. Herbert Norman Warren, O.B.E., Director of the Meteorological Branch the Australian Public Service since 1940, died at the age of 62 on 5 August 1950. played an important part in the furtherance of international co-operation in teorology and in 1947 was chairman of the committee charged with the responsi-ty of drafting the Convention for the new World Meteorological Organisation.

Centenary celebrations 1950

The Centenary Celebrations of the Society were held at Oxford and London ween Tuesday 28 March and Monday 3 April 1950.

Some 350 members and guests, including 60 visitors from 24 countries, took t in the celebrations, which were arranged by a Committee of Council con-ing of :

Centenary committee

Chairman : Sir Robert Watson-Watt, C.B., F.R.S.

Secretary : O. M. Ashford, B.Sc., A.Inst.P.
F. J. Scrase, O.B.E., M.A., Sc.D., F.Inst.P.
J. M. Stagg, O.B.E., M.A., D.Sc.
R. C. Sutcliffe, O.B.E., Ph.D.
W. M. Witchell, B.Sc.

Symposia Sub-Committee
O. M. Ashford, B.Sc., A.Inst.P.
P. A. Sheppard, B.Sc., F.Inst.P.
R. C. Sutcliffe, O.B.E., Ph.D.

Oxford Representative : A. W. Brewer, Ph.D., A.Inst.P.

The following is a brief summary of the main events. A full account is given in the *Centenary Proceedings of the Royal Meteorological Society*, 1950.

28 - 31 March. Meteorological Symposia held at the Clarendon Laboratory, Oxford on the following subjects :
' Radiation and its effect on the troposphere and lower stratosphere.'
' Physics of cloud and precipitation.'
' The structure of weather systems.'
' The general circulation.'
' Climatic change.'
' Meteorology and the community.'

1 April (afternoon) Visit to Meteorological Office, Harrow.

1 April (evening) Conversazione in the Society's rooms.

3 April (morning) Symposium on ' Atmospheric Turbulence ' in the Department of Meteorology, Imperial College of Science and Technology, followed by inspection of the department.

3 April (afternoon) Centenary Meeting of the Society in the Society's rooms.

3 April (evening) Centenary Dinner at the Connaught Rooms, W.C.2.

Several related events also took place : the *Science of Weather* exhibition at the Science Museum ; evening discourses on meteorological topics at the Physical Society's Annual Exhibition of Scientific Instruments ; radio-broadcast on 5 April by the British Broadcasting Corporation from the Society's rooms entitled *A hundred years of weather*. The President, Sir Robert Watson-Watt, gave radio talks on ' One hundred years of weather watching ' and ' The earth's atmosphere.'

The Council wish to record their deep gratitude to the members of the Centenary Committee, other Fellows, and office staff for their work in carrying through all the events so successfully. Particular tribute is paid to the untiring work of the Secretary of the Centenary Committee, Mr. O. M. Ashford, which contributed in great measure to the success of the Centenary celebrations.

Meetings

Sir Robert Watson-Watt, C.B., F.R.S., delivered the Presidential Address ter the Annual General Meeting on 25 January 1950, his subject being 'The olution of meteorological institutions in the United Kingdom.'

The ordinary meetings of the Society were as follows ; unless otherwise stated ey were held in the Society's rooms :

22 February. The following paper was read : 'The bright band — a phe- menon associated with radar echoes from falling rain,' by J. E. N. Hooper and . A. Kippax (*Q.J.*, **76**, p. 125). This was followed by a discussion on 'Radar hoes' opened by Messrs. R. F. Jones, J. E. N. Hooper and A. A. Kippax.

19 April. Professor P. M. Blackett, F.R.S., delivered the Symons Memorial ecture, his subject being 'Theories of the origin of cosmic rays.'

17 May. The following paper was read : 'Variations in thunderstorm severity Great Britain,' by J. S. Forrest (*Q.J.* **76**, p. 277). This was followed by a talk : 'Recent work of the Thunderstorm Census Organisation' by its Director, Morris Bower (*Q.J.*, **76**, p. 455) and a general discussion on 'Thunderstorms Great Britain.'

21 June. The following paper was read : 'Types and Spells of Weather around ie year in the British Isles : Annual trends, seasonal structure of the year, singu- rities' by H. H. Lamb (*Q.J.*, **76**, p. 393).

22 November. The following papers were read and discussed :

) 'Notes on the measurement and estimation of atmospheric radiation' by . D. Robinson (*Q.J.*, **76**, p. 37), (b) 'Evaporation over the British Isles' by . L. Penman (*Q.J.*, **76**, p. 372).

20 December. The following papers were read and discussed :

) 'The application of the similarity theory of turbulence to atmospheric diffusion' y G. K. Batchelor (*Q.J.*, **76**, p. 133), (b) 'Advective temperature change in the 00-700 mb and 700-500 mb layers' by J. M. Craddock (*Q.J.*, **77**, p. 51).

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12 July. The Summer Meeting of the Society was held at the Central Fore- sting Office, Dunstable, by courtesy of the Director, Meteorological Office. me 90 members and their friends were welcomed at 2.30 p.m. by the Director, r Nelson K. Johnson, K.C.B., and a reception committee. The party spent a ost enjoyable and instructive afternoon.

18 October. A joint discussion meeting with the Scientific Film Association as held on 'The use of films in meteorology.' Short contributions were read by : r David Brunt, J. S. Farquharson, P. J. Meade, D. Segaller, J. D. Chambers, R. W. Jenkins ; followed by a display of meteorological films.

14 December. A joint meeting with the Royal Aeronautical Society was held at the Institute of Civil Engineers. Dr. G. S. Hislop read a paper on 'Investigation of gusts,' describing work done on the atmospheric phenomenon of clear-air bumpiness. The Society was represented on the platform by Assistant Professor P. A. Sheppard.

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4 December. In addition to the above meetings of the Society, the President, Sir Robert Watson-Watt, C.B., F.R.S., gave a Popular Lecture on 'Wireless in weather watching' at the Royal Institution. The lecture was open to Sixth Form students of whom some 250 were present, by arrangement with the education authorities of the London, Middlesex and Surrey County Councils.

Meetings of provincial centres

Scottish Centre.

Meetings were held at Edinburgh University.

14 February. S. E. Virgo, Senior Meteorological Officer, Pitreavie, spoke on 'Some meteorological aspects of long-distance aviation.'

17 March. By the courtesy of Mrs. Margaret Buchan, extracts were read from the Scrap Book of Dr. Alexander Buchan, the distinguished Scottish meteorologist who was Secretary of the Scottish Meteorological Society from 1860 to 1907.

17 May. By the courtesy of the Officer Commanding, members of the Scottish Section visited the R.A.F. Station at Leuchars, Fife.

23 October. Meteorological Films, including Dr. Vincent J. Schaefer's 'Seeding the clouds,' were shown.

17 November. Dr. R. H. Golde spoke on 'The lightning discharge.'

Midland Centre.

31 January. J. K. Bannon spoke on 'Thunderstorms.'

28 February. The Annual General Meeting was held and was followed by Meteorological Films.

Canadian Branch.

The annual meeting of the Canadian Branch was held on 19 January in Toronto.

In view of the rapid growth of membership throughout the country, meetings were held in various cities as follows :

Toronto

26 January. P. D. McTaggart-Cowan gave the presidential address on 'The jetstream.'

23 February. R. W. Rae gave a talk on 'Arctic Experiences' or 'Life ins at forty below.'

30 March. Dr. H. Reuter spoke on 'Post-war meteorology in Central ope,' and Percy Saltzman on 'Operation Icecapade.'

27 April. 'The Royal Meteorological Society Centenary Celebrations and sent trends in meteorological research in the United Kingdom' was the title talk given by W. L. Godson.

25 May. Dr. Sverre Petterssen read a paper on 'Some aspects of the eral circulation of the atmosphere.'

28 September. C. H. Smiley presented a paper on 'Atmospheric refrac- at low altitudes.'

26 October. 'Building and climate' was the subject of a talk by R. F. get.

30 November. F. L. K. R. Greenway spoke on 'My experiences with ic flying weather.'

ntreal

19 April. Dr. H. Reuter spoke on 'Post-war meteorology in Central ope.' J. S. Marshall contributed 'Coalescence and heat transfer in the elopment of precipitation' and J. H. Heierman 'Application of meteorology relation to recent developments in aircraft.'

couver

22 May. Dr. H. Reuter spoke on 'Post-war meteorology in Central ope.'

nipeg

2 June. There was an informal meeting at which Dr. H. Reuter spoke.

Awards

The following Awards were made by the Society and presented to the recipients he Centenary Meeting on 3 April :

Buchan Prizes, in respect of the period 1944-1948.

To Charles Henry Brian Priestley, M.A., F.Inst.P., for contributions to the y of dynamical meteorology contained in the following papers which have eared in the *Quarterly Journal* of the Society, 'On the frictional influence of zontal wind shear on vertical motion.' (**72**, p. 284) ; 'Dynamical control of spheric pressure, I' (**73**, p. 65) ; 'Dynamical control of atmospheric pressure, (**74**, p. 67) ; and (in joint authorship), 'An investigation of subsidence in the atmosphere,' (**73**, p. 43).

To Reginald Cockcroft Sutcliffe, O.B.E., Ph.D., for his outstanding contri- on to the study of vertical motion in relation to the atmospheric fields of pressure

and temperature, as given in his paper, ' A contribution to the problem of development,' (*Quart. J. R. Met. Soc.*, **73**, p. 370).

(b) *Hugh Robert Mill Medal and Prize.*

To Hugh Robert Mill, D.Sc., LL.D., for his outstanding contributions to the science of meteorology with particular reference to rainfall as detailed in the following summary of his career.

Hugh Robert Mill was Director of the British Rainfall Organization and Editor of *British Rainfall* and *Symons's Meteorological Magazine* from 1901 until he retired in 1919, when the Organization was incorporated in the Meteorological Office.

During this period he applied his authoritative geographical knowledge and his experience to open up new lines of research in the study of rainfall over the British Isles. In addition to reports included in *British Rainfall* and *Symons's Meteorological Magazine* he contributed, between 1904 and 1916, chapters on the rainfall of various counties to the Geological Survey Memoirs and set out the results of some of his researches in other papers, including the following :

' On the distribution of mean and extreme annual rainfall over the British Isles.' (*Proc. Inst. Civ. Eng.*, **155**, p. 1).

' Map-studies of rainfall.' (*Quart. J. R. Met. Soc.*, **34**, p. 65).

' Report on the rainfall in the Exe Valley.' (*Geogr. J.*, 1909, p. 630).

' Unprecedented rainfall in East Anglia, Aug. 25-26, 1912.' (*Quart. J. R. Met. Soc.*, **39**, p. 1).

' Isomeric rainfall maps of the British Isles ' (with Carle Salter). (*Quart. J. R. Met. Soc.*, **41**, p. 1).

Already distinguished as a geographer and for his knowledge of the Antarctic, Hugh Robert Mill also became the authority on British rainfall and was consulted by Government Departments and other bodies. He aroused great interest in this subject by his fascinating presentation of the facts, and of their interpretation, in lectures and in contributions to scientific journals. He was a source of inspiration in rainfall research. His work was of great importance in many practical aspects of climatology, agriculture, water supply and water power, as well as in pure science.

(c) *Darton Prize.*

1. Rodney B. M. Levick, for his paper ' 50 years of English weather ' (1949) *Weather*, **4**, p. 206.

2. James M. Craddock, M.A., for his paper ' The development of cumulus cloud ' (1949), *Quart. J. R. Met. Soc.*, **75**, p. 147.

(d) *Sir Napier Shaw Competition*, 1949.

The following awards were announced in January 1950 : Junior Section, first prize to I. H. Chuter; second prize to D. M. Windus; third prize to J. D. Ovenden; Senior Section, first prize to J. E. Bowman (two other entries by the same competitor also highly commended) ; second prize to Miss J. Whillis.

Publications

The Centenary year was made the occasion to change the format and improve appearance of the *Quarterly Journal*. The printing of the 1950 volume, and of *Centenary Proceedings of the Royal Meteorological Society*, 1950, was entrusted the Salisbury Press and the Council take this opportunity of expressing their preciation of the co-operation and care which have resulted in so pleasing a duction. The four numbers of the *Journal* together contain 513 pages, an in-ase of 66 on the 1949 volume and an indication of the growth which is to be ected in coming years. The Editing Committee under whose care the *Journal* produced was also responsible to Council for the publication of a volume of pages entitled *Centenary Proceedings of the Royal Meteorological Society*, 1950, taining 28 papers read at the Centenary Symposia and accounts of other gather-s. Thus the material published during the year under review amounted to rly 800 pages. Council are greatly indebted to the Editor, Asst. Prof. P. A. ppard, and his Committee for their services in carrying out this work.

Weather, the Society's monthly popular magazine, continued to prosper and n new readers. A Centenary number of double the normal length was published March and achieved a world-wide distribution, notably in the U.S.A., where h of the 4,000 members of the American Meteorological Society received a copy.

The Editors of *Weather* were responsible for organizing a petition for the toration of Airmet broadcasts, for which over 20,000 signatures were received, l it is hoped that some restoration of this service may result in 1951. During year Dr. A. R. Meetham, a foundation Editor, and Instr. Cdr. F. L. Westwater igned from Editorship, the Editing Committee at the end of the year consisting Messrs. O. M. Ashford (Chairman), D. Chilton, F. H. Ludlam, C. D. Ovey l J. S. Sawyer. The Council wish to record their great appreciation of the in-uable service rendered to the Society by these Fellows.

Under the able editorship of Mr. E. L. Hawke a new and enlarged edition of late Richard Inwards's book *Weather Lore* was printed by Messrs. Rider and Co. behalf of the Society and appeared at the time of the Centenary Celebrations. e new edition has already achieved a considerable popularity with a wide reader-p in addition to the Society's membership. The Council wish to express their preciation of the work carried out by Mr. E. L. Hawke.

Yet another enterprise during the Centenary year was the publication of a ies of 24 postcards with photographs depicting the main cloud-types and re-duced from the Clarke Collection. There has been a very wide demand from ucational establishments and students for this postcards series and it undoubtedly ms a valuable addition to the Society's publications popularizing the study of teorology. The Council wish to record their appreciation to Mr. F. H. Ludlam, Society's Curator of Photographs, for supervising the work of publication of s series.

Staff

The Centenary Celebrations added very considerably to the normal volume of rk and the Council are greatly indebted to their assistant secretary, Mr. M. E.

Shoenberg, and the other members of the staff, for the loyal and unsparing service which they gave in order to ensure the success of the celebrations.

The Council were sorry to lose on retirement, on 31 May, Mrs. J. A. Cook, who had served very efficiently as the *Weather* clerk almost from the inception of this publication. At an informal farewell party the Editors of *Weather* expressed their appreciation of Mrs. Cook's services by presenting her with a suitable gift. The Society lost at the same time the services of the library assistant, Miss I. Dickinson (appointed 22 September 1949), who took up a post with the Ministry of Town and Country Planning.

New additions to the staff were Miss V. C. Jones (appointed 1 January) who assisted in the organisation of the Centenary celebrations, and on Mrs. Cook's retirement took over the duties of *Weather* clerk. Mrs. J. A. Blackmore, A.L.A., was appointed to the new post of assistant librarian on 1 June.

At the close of the year in addition to the above, clerical staff acting under the Assistant Secretary's direction consisted of Miss E. O. Faber (accountant, appointed 1 February 1949), Miss M. Kline (secretarial assistant, appointed 1 November 1949), and Mr. R. H. Mason (junior publications clerk, appointed September 1949).

It is gratifying to note that there have not been so many staff changes in 1950 as in previous years.

Vacation courses in meteorology

A pioneer course in this country in 'Synoptic and observational meteorology' was held in collaboration with the Council for the Promotion of Field Studies from 6-13 September at their Malham Tarn Centre in the West Riding of Yorkshire. Some 40 students (including 6 ladies) of widely varying ages and qualifications, but all united in their enthusiasm for the subject, attended the course. The lecturers were Messrs. J. Paton (in charge), C. D. Ovey, and N. Pye. In addition to lectures and observational work, evening discussions including a Brains Trust were held.

The outstanding success of the pioneer course has justified the planning of further and extended courses in 1951, and the formation of a joint committee with the Council for the Promotion of Field Studies.

The Council wish to record their thanks to Professor G. Manley, as the initiator of the whole project, to the lecturers, to the Director of the Meteorological Office for his very ready co-operation in providing the instruments required and the daily synoptic data from Preston, and finally to the Director and staff of the Central office of the Council for the Promotion of Field Studies for much hard work and enrolment of the Students ; to the Warden and Asst. Warden of the Malham Tarn Field Centre and their staff for providing such excellent accommodation and catering facilities. A special tribute must be paid to Mr. C. D. Ovey (chairman of the Malham Tarn Committee), who in addition to lecturing was responsible for much of the considerable work entailed in making the preliminary arrangements for the course.

To quote from the February (1951) *Weather* : ' We may hope that the future ill hail the Centenary year, 1950, as the year in which full recognition was given the importance of fostering meteorology as a branch of popular knowledge.'

Canadian branch

As already indicated in the membership section of the report, the Canadian ranch made very rapid strides in 1950 and nearly doubled its membership during iis period. Numerous meetings were held in various cities and very close contact as maintained with the meteorological staff and personnel of the Royal Canadian ir Force, from whom most of the new members were recruited. Another activity ndertaken by the Canadian Branch was the publication for distribution to Canadian iembers of a series of papers on subjects of wide interest. These are available in ie Society's library.

The Canadian Branch participated in the Centenary Celebrations by sending the ' Science of Weather ' exhibition a collection of Canadian meteorological istruments, climatological maps and photographs. The Canadian delegate at the entenary Celebrations was Dr. W. L. Godson, who read a paper at the Oxford ieeting.

These outstanding achievements in progress were only made possible by the nremitting and devoted work of the Canadian President, Mr. P. D. McTaggart-owan, the Secretary, Mr. F. Benum, the Canadian executive committee, and many ther Canadian members, to whom the Council wish to record their deep ppreciation of their services. It is a great satisfaction to see this learned Society so rmly establishing itself in Canada.

British rainfall organization fund

1950 was the 94th consecutive year of regular records at the meteorological ation at 62 Camden Square, N.W.1., the property transferred by the British ainfall Organization to the Society in 1923. Mr. and Mrs. A. H. Dodd continued act efficiently as resident observers, and to supply daily observations by tele-ione to the Meteorological Office as well as written reports for inclusion in the *Ionthly Weather Report*. Mr. D. L. Champion has been responsible for super-sing the work at the station, and the Council wish to express their appreciation his valuable services.

On the recommendation of the British Rainfall Organisation Committee the rst award of the Hugh Robert Mill Medal and Prize was made to Dr. H. R. Mill. he citation is given under the Awards Section of this report. The design for the edal and the preparation of the dies was carried out by Messrs. Pinches.

Phenological work

Hopes for the continuance of the annual phenological survey have not been lfilled and the usual forms have not been sent out.

Library

The vacancy of Honorary Librarian was filled in January 1950 by Dr. E. T. Eady.

Owing to the pressure on space, it had become necessary to reorganize the Society's Library. The aim is to retain only those books which experience has shown to be of the type likely to be consulted. These include modern text-books and monographs dealing with basic physical science, with meteorology and allied sciences such as oceanography, complete series of meteorological and geophysical periodicals, and climatological monographs, atlases and books of averages. Complete sets of the publications of the Society and of the Meteorological Office will also be retained, as will all material of real historical value. It is hoped that this policy will result in the formation of a more compact and practically useful library, of which Fellows will be able to make full use. As the more modern books and periodicals will be placed on the lower shelves, direct inspection and 'browsing' will be facilitated.

A large part of the library consisted heretofore of long series of meteorological yearbooks and weather reports from all parts of the world. Experience had shown that these were practically never consulted by Fellows, and their presence on the shelves prevented the orderly arrangement of the more useful books. A working party was organised by the Librarian and carried out the considerable work involved in segregating and listing the books for disposal. In the meantime an American buyer made a substantial offer for the whole of the redundant material, which was shipped to U.S.A. in the autumn, after certain portions had been microfilmed by the Meteorological Office. The Society has in this way helped in the dollar-export drive, and benefited financially thereby.

Maisonette

There has again been no change in the tenancies of the three flats on the upper floors of the Society's house in Cromwell Road.

Finance

Expenditure during 1950 has inevitably been abnormal. The Council felt that celebration of the Society's Centenary on a somewhat ambitious scale would be agreeable to most members, and that, although its effect on the finances might be temporarily depressive, much benefit would accrue in the long run from the stimulus obtained.

The occasion seemed proper for a thorough rehabilitation of the Society's quarters. In response to a special Centenary appeal to members for funds to meet the cost, there were 195 contributors, the total sum contributed being £294. The final cost of the work carried out was £950.

The work included alterations necessary for the provision of a much needed cloak-room, which has been constructed in the basement of the premises and in which lavatory and hot-water accommodation has been installed.

One hundred new metal-framed chairs, of a modern type, which can be tacked,' have been purchased for the meeting room at a cost of £130.

Special expenditure in connection with the Centenary meetings amounted to proximately £260 of which about one half was the cost of entertaining the Society's ests, many of whom came from overseas. The remainder represents expenditure cidental to the general arrangement of the several functions and includes clerical sistance and much special printing.

It may be mentioned that the statement of receipts and expenditure specifically lating to the Oxford meetings shows a small favourable balance.

With regard to the general finances, a gratifying increase in the revenue from les of publications may be noted.

One interesting item is the sale to an American buyer of two complete sets of e *Quarterly Journal* for a substantial sum in dollars. Only 4 complete sets now main in the Society's store.

The Council record with gratitude an allocation to the Society of £800 from e Government Grant-in-aid of Publication Fund with an addition of £150 for rary expenditure. The action of the Royal Society Committee in making this nerous allocation is greatly appreciated by Council, the more because the cost of inting and publication has continued to rise and provides a troublesome problem r the future.

As a means of increasing immediately the annual income of the Society by a bstantial amount, Council have put before Fellows the great advantage to the ciety if their annual subscriptions are paid under a formally executed ' Covenant ' tending over a minimum period of seven years. These subscriptions, being rmally paid from income which has borne income tax, are then classified by the evenue Authorities as *income of the Society, taxed at source* ; and the Society, not ing subject to income tax, is able to claim repayment of the appropriate amount tax.

175 Fellows have responded at present and the anticipated refund will be in e neighbourhood of £400 per annum, at existing tax rates.

The magazine *Weather* continued to show satisfactory returns, and the nett nual cost to the Society of the copies provided without charge to members is pected to be well below £300 in future years.

Weather Lore (by the late Richard Inwards). The copyright in this book passed the Society in 1937 under the will of the author, a former president. The Council, ving resolved that the issue of a new edition should be undertaken, arranged at its appearance should coincide with the Centenary celebrations. This project cessitated the preliminary expenditure of £400 and by Council's decision, money ng idle in the Research Fund was utilised to finance the publication. Accordingly e fund has been renamed The Research and Special Purposes Fund and it will imately benefit from the anticipated profits. Sales and royalties have amounted,

so far, to approximately £240. Members can help the Fund substantially by recommending the book to their friends.

Other subsidiary funds :

Staff pension fund. With the decease of Mr. Hampton Brown no immediate calls on this fund are in prospect, but the fund will be built up steadily to meet future contingencies.

The repairs and maintenance fund has been completely exhausted by the work done on the Society's house, referred to earlier in the report.

President's fund. This fund has been merged in the Awards Fund, the purpose for which it was established having ceased to operate.

British Rainfall Organization fund. There have been quite exceptionally heavy calls on this Fund during the year. The institution of the Hugh Robert Mill Medal involved the preliminary expenditure of one hundred guineas for the design of the medal ; and further expenditure in respect of the actual production.

The condition of the premises at 62 Camden Square was professionally surveyed in 1949, and substantial repairs were found to be necessary to prevent serious deterioration of the property. These were put in hand and have cost £350.

Napier Shaw memorial fund. There have been a few additions by donation, and these together with the interest from the invested capital have increased the total slightly.

The position of the Fund is being reviewed by an *ad hoc* committee.

At the conclusion of the first century of its corporate existence the Society finds itself the owner of its headquarters, the very valuable freehold house 49 Cromwell Road, South Kensington, a comprehensive library, an invested reserve of modest dimensions (capital value about £2,000) substantial reserves for the liabilities respectively of compounded Fellowships (£2,500) and staff pensions (£1,575), and a good stock of past publications of considerable sales value.

Though the present financial stringency foreshadows difficult times ahead, the Council believe that the Society may begin the second century of its activities in a spirit of justifiable optimism with regard to finance.

Legacies. The Society has received a legacy of £20 under the will of the late Major A. F. Bowker.

The Society will eventually benefit under the will of the late Mr. A. Hampton Brown to the extent of £1,000. The testator's desire, without imposing any trust, was that the bequest should be used chiefly for the maintenance of the library of the Society.

The Society will also benefit under the will of the late Dr. H. R. Mill.

These legacies are greatly appreciated by the Council.

Instruments

All the meteorological instruments on loan from the Society are reported to in good condition and satisfactorily serving their purpose.

Donations

The Council record their gratitude to Sir George Simpson, K.C.B., F.R.S. the gift of a collection of slides illustrating snow crystals, and to those authors o have sent copies of their papers, which will be a valuable addition to the Society's brary.

Scientific congress — German Geophysical Society and Meteorological Society of Hamburg

The Society was represented by Dr. R. Cranna and R. J. Murgatroyd at the teorological sessions of this Congress held at Hamburg on 23 to 28 October 1950.

Representatives

The following is a list of the Society's representatives on other scientific bodies :

ophysical Committee of the Royal Astronomical Society.
Professor S. Chapman, appointed 1939.

itional Committee for Geodesy and Geophysics.
Professor S. Chapman, appointed 1946 ;
Mr. E. Gold, appointed 1949.

ard of the Institute of Physics.
Dr. G. D. Robinson, appointed 1949.

itish National Committee for Physics.
Dr. F. J. Scrase, appointed 1948.

search Committee, British Gliding Association.
Dr. A. E. Slater, appointed 1946 ;
Professor G. Manley, appointed 1950.

e Meteorological Committee of the University of Cambridge.
Assistant Professor P. A. Sheppard.

lvisory Committee for the Meteorological Office, Edinburgh.
Mr. J. Paton, appointed March 1949.

entific Film Association.
Mr. P. J. Meade.

ssiot Committee.
The President, Sir Robert Watson-Watt, C.B., F.R.S.

STATEMENT OF RECEIPTS AND PAYMENTS

RECEIPTS	£ s. d.	£ s. d.
Balance from 1949. Cash at Westminster Bank Ltd.	16 7 11	
Cash on deposit at P.O.S.B.	7 10 0	
		23 17 11
Subscriptions for 1950	3756 5 7	
,, ,, former years	370 18 3	
,, ,, 1951	186 0 9	
		4313 4 7
Entrance Fees		151 4 0
Dividends and Interest		70 9 4
Sale of publications, advertisements, etc.		1905 4 6
Grant made by the Royal Society from Government Publications grant (includes £100 voted in December 1949)		750 0 0
Amount transferred from Life Composition Fund		300 0 0
Amount transferred from Staff Pension Fund		130 0 0
Rent of Maisonette		323 7 8
Legacy from A. F. Bowker		20 0 0
Donation		1 0 0
Centenary Special Appeal		294 9 1
Contribution for Admistrative purposes from the British Rainfall Organisation Fund		5 0 0
		£8287 17 1

1 *March* 1951

OR THE YEAR ENDING 31 DECEMBER 1950

PAYMENTS

	£ s. d.	£ s. d.
blications, etc.—		
arterly Journal, four numbers October 1949 to July 1950 inclusive	1667 6 11	
Illustrations	281 5 2	
Reprints of Papers	182 11 0	
Reviews	24 1 0	
Advance to *Weather* on account of members' copies	300 0 0	
Bibliography of Meteorological Literature	254 0 0	
Second-hand Journals	50 8 5	
		2759 12 6
nting—		
General printing	64 0 6	
Stationery	250 9 6	
Books and binding	74 5 1	
		388 15 1
fice Expenses—		
Salaries, Pensions and Insurance	1897 11 8	
Cleaners and Insurances	234 5 9	
Furniture and minor repairs	214 7 3	
Auditing Accounts for 1949	21 0 0	
Telephone charges	73 6 9	
Petty expenses	109 1 1	
Postages	418 10 3	
		2968 2 9
al, Lighting, Insurance, etc.—		
Coal and coke	55 3 9	
Lighting and gas	53 9 11	
Water rate	11 6 2	
Insurance	27 11 7	
		147 11 5
etings—		
Expenses and refreshments at meetings	46 7 4	
Midland Centre meeting expenses	10 0 0	
		56 7 4
isonette—		
General Rate to 31.3.51	109 6 6	
Water Rate to 31.3.51	11 12 2	
Repairs	39 0 0	
		159 18 8
scellaneous Expenditure—		
Rebate of subscriptions to Canadian Branch	111 15 6	
Rebate of entrance fees to Canadian Branch	15 9	
Clarke Collection reproductions (Cost less sales)	117 15 0	
		230 6 3
Centenary Expenses		262 10 11
Transferred to Staff Pension Fund		95 0 0
Purchase of £400 2½% Consols		278 0 0
Transfer to Research and Special Purposes Fund A. F. Bowker legacy		20 0 0
Buchan Prize (two awards)		37 16 0
Transfer to Repairs and Maintenance, Centenary Appeal	294 9 1	
Transfer to Repairs and Maintenance Fund	482 9 10	
		776 18 11
h at Bank—		
Westminster Bank and in hand		106 17 3
		£8287 17 1

Examined, compared with vouchers, and found correct.

NCER HOUSE, SOUTH PLACE, LONDON, E.C.2

BALL, BAKER & CO.,
Chartered Accountants.

ASSETS AND LIABILITIES,

LIABILITIES

	£ s. d.	£ s. d.
Subscriptions paid in advance	186 0 9	
Subscriptions to publications paid in advance	146 11 6	
		332 12 3
Accounts oustanding—		
Quarterly Journal and reprints, October 1950	630 10 4	
Sundry Expenses	141 15 9	
		772 6 1
Excess of Assets over Liabilities		
This excess is exclusive of the value of the Library and Stock of Publications.		8137 13 2
		£9242 11 6

1 *March* 1951

LIFE COMPOSITION FEES FUND,

RECEIPTS

	£ s. d.	£ s. d.
Balance from 1949 — Cash at Westminster Bank Ltd.	64 12 11	
Cash on deposit in P.O.S.B.	5 2 6	
		69 15 5
Composition fees received during 1950		310 0 0
Interest on Investments		84 3 6
		£463 18 11

1 *March* 1951

DECEMBER 1950

ASSETS

	£	s.	d.	£	s.	d.
overnment of the Commonwealth of Australia 4 per cent Registered Stock, 1955-70, £649 1s. 7d. @ 102½ XD.	665	6	1			
nding Stock, 4 per cent 1960-90, £200 @ 107¼	214	10	0			
3½ per cent Conversion Stock £900 @ 96¼	866	5	0			
2½ per cent Consolidated Stock £400 @ 71 XD	284	0	0			
				2030	1	1
ote : The above stocks are valued at prices estimated by the Society's Brokers						
dvance for publication of "*Weather*" Magazine				300	0	0
bscriptions unpaid amounting to £1072 4s. 9d. estimated at ..	650	0	0			
ccounts due for publications	340	11	6			
				990	11	6
rniture and Fittings	418	13	8			
struments	16	8	0			
timated value (in 1925) of Society's House .. 49 Cromwell Road	5375	0	0			
				5810	1	8
eposit with London Electricity Board	5	0	0			
lance Cash at Westminster Bank Ltd.	106	17	3			
				111	17	3
				£9242	11	6

Examined and securities seen,
BALL, BAKER & CO.,
Chartered Accountants.

DECEMBER 1950

PAYMENTS

	£	s.	d.	£	s.	d.
ansferred to General Account				300	0	0
lance — Cash at Westminster Bank Ltd*	160	0	11			
Cash on deposit with P.O.S.B.	3	18	0			
				163	18	11
				£463	18	11

he Society holds on account of this Fund—
£800 2½% Consolidated Stock.
£800 3½% Conversion Stock and
£1,200 3% Defence Bonds.

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

STAFF PENSION FUND,

RECEIPTS

	£	s.	d.
Balance from 1949 — Cash at Westminster Bank Ltd.	8	10	5
Interest on Investments	57	5	0
Transfer from General Account	95	0	0
	£160	15	5

1 *March* 1951

RESEARCH AND SPECIAL

RECEIPTS

	£	s.	d.
Balance from 1949 — Cash at Westminster Bank Ltd.	48	19	1
Interest on Investments	17	5	0
Sale of 2½% Consols to General a/c	278	0	0
Transfer from General a/c legacy A. F. Bowker	20	0	0
Receipts for *Weather Lore*	179	2	11
	£543	7	0

1 *March* 1951

AWARDS FUND,

RECEIPTS

	£	s.	d.
Balance from 1949 — Cash at Westminster Bank Ltd.	15	0	0
Donations for Napier Shaw prizes	25	0	0
Transfer from President's Fund	13	13	0
	£53	13	0

1 *March* 1951

DECEMBER 1950

PAYMENTS

	£	s.	d.
s. A. Hampton Brown — gift	25	0	0
ınsfer to General Account	130	0	0
ance — Cash at Westminster Bank Ltd. *	5	15	5
	160	15	5

The Society holds on behalf of this Fund—
£800 3½% War Loan.
£300 3½ Conversion Stock.
£750 2½% Consolidated Stock.

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

URPOSES FUND, 31 DECEMBER 1950

PAYMENTS

	£	s.	d.
nting and packing of *Weather Lore*	423	3	6
ty expenses		18	6
ance — Cash at Westminster Bank Ltd.*	119	5	0
	£543	7	0

The Society holds on behalf of this Fund—
£250 3½% War Loan.
£100 3½% Conversion Stock

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

DECEMBER 1950

PAYMENTS

	£	s.	d.
ɔier Shaw Prizes	25	0	0
st of advertising and printing for prizes refunded to "*Weather*"	12	0	0
ance — Cash at Westminster Bank	16	13	0
	£53	13	0

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

REPAIRS AND MAINTENANCE

RECEIPTS

	£ s. d.	£ s. d.
Balance from 1949 — Cash at Westminster Bank Ltd.	32 9 6	
Cash on deposit at P.O.S.B.	100 8 8	
		132 18 2
Interest on Post Office Savings Bank deposit a/c		1 5 0
Transfer from General a/c	482 9 10	
Transfer from Centenary Special Appeal a/c	294 9 1	
		776 18 11
Transferred from Maisonette a/c		39 0 0
		£950 2 1

1 *March* 1951

ROYAL SOCIETY PROCEEDINGS

RECEIPTS

	£ s. d
Balance from 1949 — Cash at Westminster Bank Ltd.	25 2 0
Deposits received	3 19 10
	£29 1 10

1 *March* 1951

ND, 31 DECEMBER 1950

PAYMENTS

	£	s.	d.
eral Repairs	948	17	1
nce — Cash on deposit at P.O.S.B.	1	5	0
	£950	2	1

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

ND, 31 DECEMBER 1950

PAYMENTS

	£	s.	d.
of reprints of Proceedings	8	15	7
nce — Cash at Westminster Bank Ltd.	20	6	3
	£29	1	10

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

BRITISH RAINFALL ORGANIZATION

RECEIPTS

	£	s.	d.
Balance from 1949 — Cash at Westminster Bank Ltd.	225	17	9
Rent of 62 Camden Square	80	0	0
Sale of *Rainfall Atlas*	4	8	9
Interest on Investments	123	18	10
Balance due to Westminster Bank Ltd.*	147	4	11
	£581	10	3

NAPIER SHAW MEMORIAL FUND

RECEIPTS

	£	s.	d
Balance from 1949 — Cash at Westminster Bank Ltd.	45	11	8
Donations	5	0	0
Interest on Investments	12	5	0
	£62	16	8

1 *March* 1951

UND, 31 DECEMBER 1950

PAYMENTS

	£	s.	d.
yment to tenant for observational work to 31.12.50	52	0	0
pairs	350	2	6
urance	1	14	6
lephone	10	4	1
gh Robert Mill Prize	10	0	0
sign for Medal	105	0	0
es for medal	32	17	0
ansfer to A a/c	5	0	0
ty expenses	14	12	2
	£581	10	3

The Society holds on behalf of this Fund—
£750 3½% War Stock, £100 3% Defence Bonds
and £3,097 10s. 0d. British Transport 3% Guaranteed Stock,
also the Freehold of 62 Camden Square.

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

DECEMBER 1950

PAYMENTS

	£	s.	d.
ance — Cash at Westminster Bank Ltd.*	62	16	8
	£62	16	8

The Society holds on behalf of this Fund—
£350 3½% Conversion Stock 1961.

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

PRESIDENT'S FUND,

RECEIPTS

	£ s. d.
Balance from 1949 — Cash at Westminster Bank Ltd.	13 13 0
	£13 13 0

1 *March* 1951

SYMONS MEMORIAL FUND,

RECEIPTS

	£ s. d.	£ s. d.
Balance from 1949 — Cash at Westminster Bank Ltd.	56 12 6	
Cash on deposit with P.O.S.B.	51 5 0	
		107 17 6
Dividends and Interest		28 19 5
		£136 16 11

DECEMBER 1950

PAYMENTS

	£ s. d
sfer to Awards Fund	13 13 0
	£13 13 0

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

DECEMBER 1950

PAYMENTS

	£ s. d.	£ s. d.
ons Lecture		15 15 0
nce — Cash at Westminster Bank Ltd.*	69 16 2	
Cash on deposit at P.O.S.B.	51 5 9	
		121 1 11
		£136 16 11

ne Society holds on behalf of this Fund—
£650 3½% Conversion Stock and
£123 8s. 8d. 4% Consolidated Stock.

Examined and found correct,
BALL, BAKER & CO.,
Chartered Accountants.

Proceedings at meetings of the Society

Ordinary Meeting, 21 March 1951

The following candidates were declared to have been elected that day by the Council:

Fellows

Austin, Flt. Lt. Albert William, Ph.C., F.C.S., 12 Woodlands Avenue, Parkgate Road, Chester.
Baynes, Nigel Alford, M.A., 3 Saint John's Mount, Wakefield, Yorkshire.
Besley, Bryan Philip, 20 Queen Anne's Grove, Bush Hill Park, Enfield, Mddx.
Clark, F. Malcolm, 18 Longfield Avenue, Walton, Stone, Staffs.
Crisp, Alma D., Miss, 30 Kingsway, Stoke, Coventry.
Dickinson, Harold, 21 Addington Road, Reading, Berkshire.
Haughton, Leslie Frank, 16 Grove Road, Pinner, Mddx.
Howard, Norman John, " The Hut," 2 St. Anne's Drive, Herne Bay, Kent.
Kay, John Pickering, B.Sc., 202 Cheltenham Road, Longleavens, Glos.
Mani, Ramnath Subra, Air-India-International, London Airport, Lower Feltham, Mddx.
MacNeill, Hamish Campbell, Peachland, British Columbia.
Oddie, Brian C. V., 54 Castle Avenue, Ewell, Epsom, Surrey.
Samuels, Sidney Henry, 85 Childwall Park Avenue, Liverpool, 16.
Scorer, Edwin Abbott, B.Sc., 40 St. Annes Road, Belle Vue, Doncaster.
Snell, John Francis, The Gulls, Grosvenor Road, Paignton, S. Devon.
Thompson, Albert William, 21 Hazelbourne Road, Balham, London, S.W.12.
Watson, Instr. Lt., Donald Charles Frank, R.N., M.A., 72 Blacklaw Road, Dumfermline, Fife.
Webb, Eric Kenneth, c/o C.S.I.R.O., Section of Meteorological Physics, Graham Road, Highett, S.21, Victoria, Australia.

Foreign Member

Wright, Capt. Russell Edward, Ph.M., S.M., U.S.A.F., 28th Weather Squadron, A.P.O. 125, U.S. Army.

Associates

Dunlop, Gp. Capt. Gordon Parker, R.C.A.F. Station, Trenton, Ontario.
Finseth, Fg. Off. William John, R.C.A.F. Station, Trenton, Ontario.
Markham, William Edwen, 11704-126 Street, Edmonton, Alberta.
Peel, Ronald, 307 Decelles Street, Ville St. Laurent, Montreal, Quebec.
Wilson, Jack, Tappen B.C., Canada.

Student Member

Kelly, William Lamont, B.Sc., 119 Wilson Street, Bulawayo, Southern Rhodesia.

Professor E. Palmén delivered the Symons Memorial Lecture, his subject being 'The rôle of atmospheric disturbances in the general atmospheric circulation.'

Annual General Meeting, 18 April 1951

Sir Robert Watson-Watt, C.B., F.R.S., President, in the chair. The minutes of the Annual General Meeting on 25 January 1950 were confirmed.

In presenting the Report of the Council for the year 1950, the President commented on the main aspects of the Report and the happenings of importance during the year.

After remarks by the Hon. Treasurer on the financial section, and comments from e floor, the President moved that the Report be received and adopted and be printed the *Quarterly Journal*.

The motion was seconded by Dr. G. L. Hogben and carried unanimously.

Sir Charles Normand then proposed a vote of thanks to the President and the ouncil for the year 1950. Mr. A. J. Whiten seconded the motion.

Messrs. D. J. Schove and F. H. Ludlam were then appointed as scrutineers, in case allot for the election of the new Council was necessary.

The President then presented the Symons Memorial Medal to Sir Geoffrey Taylor, .A., F.R.S., Yarrow Research Professor of the Royal Society, in recognition of his classical rk on atmospheric turbulence and related subjects in meteorology.

The meeting was declared closed at 6.10 p.m.

Ordinary Meeting, 18 *April* 1951

The following candidates were declared to have been elected that day by the Council :

FELLOWS

RLINGTON, Instr. Lieut-Com. CHARLES ROY, B.Sc., R.N., Dept. of Naval Weather Service, Admiralty, Queen Anne's Mansions, S.W.1.

Y, ALFRED ERNEST, Ph.D., 4 Park Drive, Timperley, Cheshire.

NIGHT, Sqn. Ldr. THOMAS, R.A.F., Bawtry Hall, Bawtry, Doncaster.

THUR, LAWRENCE, D.F.C., 52 Selbourne Avenue, Tolworth, Surbiton, Surrey.

CKLIFF, PETER GEOFFREY, 133, Stevens Road, Singapore, Malaya.

STUDENT MEMBER

SS, MICHAEL DAVID, 42, Clevedon Road, Anerley, London, S.E.20.

Dr. E. G. Bowen, Director, Radio Physics Division, Commonwealth Scientific and dustrial Research Organization, Australia, gave a lecture on recent work by his division cloud physics, and this was followed by the showing of a film, ' Radar echoes from ecipitating clouds.'

A cordial vote of thanks for the very interesting lecture and film was proposed by . R. C. Sutcliffe and seconded by Asst. Prof. P. A. Sheppard.

Ordinary Meeting, 16 *May* 1951

The following candidates were declared to have been elected that day by the Council :

FELLOWS

NZIES, GEORGE YOUNG, 8 Grove Street, Edinburgh, (*from Student Member*).

UNT, VERNON EGERTON ROWLAND, M.A., LL.B., " St. Raphael," Bourne End, Bucks.

OWN, JOHN E., Meteorological Office, Royal Air Force, Castel Benito, M.E.A.F.(1).

NSLEY, WILFRED, 7 The Crescent, Camborne, Cornwall.

VIS, Flt. Lt., ANTONY ALFRED, 18 St. Matthews Avenue, Surbiton, Surrey.

DONALD, ALAN, 70 Buckland Crescent, Windsor, Berks.

CHOLLS, KELVIN V. W., East African Meteorological Dept., Nairobi.

OWSE, TERENCE HENRY JAMES, c/o Lloyd's Bank Ltd., Chowringhee, Calcutta.

YTHE, Flt. Lt., WILFRED VINCENT, Old Grammar School House, Skipton, Yorks.

Associates

Clodman, Joseph, D.A.F.O. Montreal Airport, Dorval, P.Q., Canada.
Munn, Robert Edward, 10 Beech Street, Halifax, N.S., Canada.
Orr, Donald James, c/o 315 Bloor Street, West, Toronto, Ontario.

Student Member

Blacktop, Graham Leonard, F.R.Econ.S., 22 Vines Avenue, Church End, London, N.3.

Sir Robert Watson-Watt, C.B., F.R.S. delivered the Presidential Address, his subject being ' Meteorology and Aviation.'

Sir Nelson Johnson proposed a vote of thanks to the President for his Address and asked that he would allow the Address to be printed in the *Quarterly Journal*. He said that Sir Robert had, as was customary with him, approached the subject from an entirely unexpected angle and had consequently drawn attention to many points which otherwise would not have been thought of at all. Sir Nelson also asked the President to accept the Society's warmest thanks for all he had done during his term of office which had extended to well over 2 years and had included the Centenary year of the Society.

In seconding the vote of thanks Cdr. C. R. Burgess said the President had chosen for his Address, a subject of wide interest. The Society was also greatly indebted to him for his most valuable contributions to the Centenary celebrations by his speeches at meetings, B.B.C. broadcasts, and articles in *The Times*.

Reviews

s Gewitter. By H. Israel with contributions by eleven others. Leipzig (Akademische Verlag, Geest & Portig), 1950. 8vo. Pp. IX, 249 ; plates, figures. No price given.

This book has been compiled by Dr. H. Israel with the object of bringing before German physicists large amount of work done on thunderstorm problems while they were cut off from the rest of scientific world during the war. It takes the form of a number of articles each dealing with one ect of the problem classified under the headings : the meteorology of the thunderstorm ; thunder- rm theory ; lightning and its radio-electric consequences ; and new results of observations. There is a chapter of abstracts of recent papers and a comprehensive bibliography covering the years 9-1949. It would serve no useful purpose to give a more detailed summary of the contents of the k, as the book itself is only a summary of papers known to most workers in the subject in this ntry. This, however, does not apply to the chapter on theories of the thunderstorm which contains ounts of four theories, three of which will be new to most readers – the fourth by H. Wichmann reviewed in the *Q.J.* 1949, **75**, p. 330.

The three new theories which will now be discussed are those of F. Rossmann, E. Wall and renkel; the first is described by Dr. Rossmann himself and the other two by the editor, Dr. Israel.

(*a*) *Rossmann's ideas*

After first discussing the theories of Simpson (the breaking-drop theory), Wilson (the influence ory) and Findeisen (the ice-splinter theory) and finding them all unsatisfactory, an opinion with ch their three authors would probably agree, Rossmann goes on to say : ' it was clear to me that whole electrical effect in a thunderstorm is bound up with atmospheric ice as a dielectric and in opinion the form taken by the solid precipitation is not of much importance. Ice-needles, snow, ipel and hail all show in the atmosphere, and essentially only there, an abnormal electrical property ch increases as the size of the ice particle increases from needles to hail, that is, with the mass of particle. I asked myself : which of the known electrical properties of solids would best satisfy se conditions ? My answer was : the electrical polarity of crystals, the pyroelectrical excitement ce.'

It will be remembered that tourmaline and other crystals show electrical polarity when heated ooled (pyroelectricity) or when they are subjected to mechanical strain (piezoelectricity). Not all tals have this property and when Rossmann wrote his article no one had been able by laboratory eriments to discover any pyro- or piezo-electrical effect in ice, but nothing daunted he devotes n pages to giving reasons why in his opinion ice should have electrical polarity. Nowhere in the cle, however, does he state how, granted that ice crystals have electrical polarity, electricity can xtracted from them and distributed throughout the cloud with the positive charge in the upper and the negative charge in the lower half. The printing of the book was held up for two years economic conditions in Germany so that when Rossmann finally corrected the proof of his article was able to add that in the meantime he had grown in a refrigerator large single crystals of ice ch show piezo-electrical action ten times as strong as tourmaline, which is the most piezoelectric tal known. But still no word of how polar-electric ice can produce the electricity in a thunder- m. With that we must leave Rossmann's article, adding however, in justice to him, that he states icitly that he is writing about an idea (*Anschauung*) and not about a theory.

(*b*) *Wall's theory*

In essentials Wall's theory is C. T. R. Wilson's ' influence theory ' except that it does not make of the normal fine-weather potential-gradient as the factor which determines that all thunder- ms have positive polarity as Wilson does.

Wall assumes without any hesitation that ice is a polar-electric crystal and that the electric axis the direction of the chief hexagonal axis so that, when newly formed at low temperatures in the osphere, ice crystals have opposite charges on their two hexagonal faces. He assumes further that rystals float with their positive faces downwards (the reason given for this is entirely unconvincing), as, under gravity, the crystals move downwards relatively to the air the positive charges on these r faces attract and absorb the negative ions while the ' positive ions stream round the crystal out an appreciable number being able to reach the negative charge on the upper faces.' Thus out any outside field the ice crystals gain a negative charge leaving a positive charge in the air. relative motion between the ice crystals and the air separates the two charges, leaving the positive in the upper cloud and carrying the negative charge on the crystals into the lower cloud. Thus sitive field is set up in the cloud quite independently of the fine-weather potential-gradient. As as this electrical field has been established in the cloud the Wilson influence effect takes charge the field goes on increasing so long as there are ascending air currents to provide new ice crystals o separate the charges. Even if we had any experimental evidence that newly formed ice crystals

are electric dipoles which float in the air with their positive poles downwards it is difficult to see how a thunderstorm would result. The theory appears to combine the weak points in Rossmann's ideas and in Wilson's theory without adding anything new which carries conviction.

(c) *Frenkel's theory*

During the last war J. Frenkel distributed copies of an English translation of an article of his which had appeared in a Russian journal. In this article a completely new theory of a thunderstorm was outlined and Israel, having obtained a copy, has made a brave attempt to explain the theory while admitting that he does not completely understand it himself.

The basis of the theory can be explained in a few words. Imagine a cloud of water particles, either liquid or solid, which for simplicity is taken to be spherical. Frenkel states that in such a cloud the cloud particles absorb negative ions from the naturally ionised air leaving the positive ions in the surrounding atmosphere. The negatively charged cloud particles settle under gravity relatively to the positive ions so that two spheres result, one of positively charged ions and one of negatively charged cloud particles, the centre of the latter being at a distance, which can be calculated, below the centre of the former. Elementary electrostatics show that the field within such a ' polarized ' cloud would be vertical and uniform and that the field outside would be the same as that which would be obtained if the cloud were replaced by an electric dipole with its positive pole upwards situated at its centre. Frenkel takes such a cloud as the model of a thundercloud and calculates the field within and without. His conclusion is that the field, in magnitude and direction, and the electrical currents which would be set up inside and outside the cloud, are in agreement with the structure of a thunderstorm found by the altielectrograph records at Kew Observatory.

I do not propose to discuss this theory in detail, but I have three remarks to make : First, the basis of the theory, that water particles obtain a negative charge in ionised air, has never been observed in the laboratory and does not agree with observations, for steady rain without strong electrical fields is always positively charged (Simpson, 1949 *Geophys. Mem.*, No. 84, p. 42). Secondly, every cloud would be charged in the way described ; yet we know that without precipitation clouds are not accompanied by an electrical field. Thirdly, a theory of thunderstorms which says nothing about ascending air currents and precipitation cannot be complete.

Dr. Israel has performed a service in making these new theories available to a wider public than they would reach in their original form. He has subjected each to a helpful criticism ; but I am not able to follow him in his appreciation of Wall's and Frenkel's theories, both of which appear to me to be quite unacceptable.

G.C.S.

New Zealand weather and climate. Edited by B. J. Garnier. Christchurch N. Z. (New Zealand Geographical Society), 1950. 8vo. Pp. vi, 154 ; illustrations, tables. No price given.

This is the first of a series of special publications sponsored by the New Zealand Geographical Society. It contains five articles on New Zealand climatology, three of which have been published previously elsewhere, whilst a useful bibliography and mean monthly temperature and precipitation data for forty stations are given in appendices. The Society, barely seven years old, is to be congratulated on its enterprise and on the attractive format of this publication.

The first three papers, discussing the development of the observational network, some characteristic weather situations, and the basic elements of New Zealand climate, call for little comment – though Mr. Watts's attempt to illustrate pictorially the effect of relief on cloud distribution in the Cook Strait area will certainly appeal to the general reader.

The most elaborate, and at the same time, the most controversial contribution is offered by Mr. Garnier himself in two papers on the vexed question of climatic classification. In the first he has applied Thornthwaite's 1931 classification to New Zealand data, and in the second he has adapted the same method to seasonal analysis. In both cases one fears that he is carrying his elaboration much further than the intrinsic nature of his data will reasonably permit.

No one is likely to question the ingenuity of Thornthwaite's analysis but we may be permitted to regret that he has bequeathed to climatology an empirical formula, relating *mean monthly* precipitation, temperature and evaporation, of the form $P/E = 11{\cdot}5\,(P/T - 10)^{10/9}$ which bears, on the face of it, an air of inherent improbability. As an analytical tool this formula must be evaluated in the light of (i) the known unreliability of short-period means, particularly of rainfall, (ii) the degree of scatter evident in Thornthwaite's Fig. 1 (*Geog. Review*, **21**, p. 637) but subsequently ignored, and (iii) the arbitrary delimitation and coarseness of grouping, *e.g.*, Humid type ; $P - E$ index 64 to 127, (*Ibid.*, p. 641) towards which the whole argument is moving. Since P is expressed in inches and T in degrees Fahrenheit, the value of $P/T - 10$ is usually so small (very rarely above 0·5) that the

rticular significance of its 10/9th power is entirely chimerical. The charge is frankly one of urious accuracy and this the reader may readily test for himself by experimenting with the formula some much simpler form, say, $P/E = 10P/T - 10$ or $P/E = 8P/T - 10$ which will give remarkly similar results, at least in the range $P/E = 0{\cdot}1$ to $1{\cdot}5$ which, to judge from the evidence ornthwaite himself presents, covers the great bulk of the instances. Mr. Garnier only underlines is failure to mould the tool to the task in hand when he admits that, in regard to temperature, he is had to resort to estimates based, be it noted, upon a standard lapse rate of 2·74°F per 1,000 ft gardless of season.

In making a seasonal summation of Thornthwaite's indices Mr. Garnier clearly feels he is taking logical step which requires no specific defence. The result is a number of striking patterns which ay, or may not, be significant. Their analysis in detail must be left to those with local knowledge, e ultimate test of any classification being, does it work ? Here we would only comment that as a ethod becomes increasingly elaborate it is all the more necessary to recall its basic assumptions, d to emphasize what Mr. Garnier himself clearly acknowledges in his final paragraph, namely, at to ignore moisture contrasts in winter over the bulk of South Island because it is ' cold ' *i.e.*, low 43°F, is seriously to underestimate the function of winter in soil and plant economy.

P.R.C.

oud Physics. By D. W. Perrie. Toronto (Toronto University Press), 1950. 8vo. Pp. 119 ; 30 plates, 4 tables. 32*s.*

The study of cloud physics has been greatly advanced in recent years and is still in active develop-ent, but there are no textbooks on the subject which include modern ideas, and meteorologists are ady to welcome any attempt to remedy this deficiency. Unfortunately, in spite of the statement the foreword to Mr. Perrie's book that ' although the title is " Cloud Physics ", the book is more an this,' in fact it is a good deal less, if it can be said to contain any physics at all. The chapter adings are all that could be desired, but the text itself is a ragged assembly of material culled from extensive list of references given at the end of the book. There are hardly any quantitative state-ents, and the physical content of the text is perhaps best illustrated by a couple of quotations. In the scription of cloud produced by ' thermal lifting,' which ' results from uneven heating of the earth solar radiation,' we are told that ' the first condensation to occur is normally in the form of extremely all water droplets, which constitute merely a haze layer. Since the air in such updraughts is usually ing quite rapidly, this haze layer is very shallow and the condensation products soon take the form cloud droplets. The shallowness of the haze layer accounts for the clear-cut appearance of the ses of cumulus clouds.' In the next chapter the statistical theory of spontaneous condensation is entioned : ' The theory of condensation without nuclei, described as *molecular condensation*, requires e joining of sufficient numbers of water molecules to form droplets. Against such condensation mpson presents a very powerful argument. Since the diameter of a water molecule is 4×10^{-8} cm, out 5,000 molecules would be required to assemble together to make a drop with a diameter of $^{-4}$ cm. It is quite unlikely that so large a number of molecules would come together by chance. any case, a drop so formed would fly apart in any atmosphere having a supersaturation of less than per cent.'

It would have been better for Mr. Perrie to have given the book a more popular appeal, without empting to design it ' primarily for readers with an active interest in meteorology,' and to have cluded many more photographs (there are thirty) and word-pictures of clouds, which he is evidently ll qualified to describe.

F.H.L.

ndon Essays in Geography. Rodwell Jones Memorial Volume, edited by L. D. Stamp and S. W. Wooldridge. London (Longmans, Green & Co., for London School of Economics), 1951. 8vo. Pp. xiv, 351 ; 72 figures. 25*s.*

This volume is published as a memorial to the late Professor L. Rodwell Jones who held the air of Geography at the London School of Economics and in that capacity shared in the work of Joint School of Geography with King's College ; in it there are seventeen essays by a group of former students and colleagues in the Joint School. These essays cover a characteristically wide ld ; two of them may be commended to readers of this *Journal* on account of their content and interest. . P. R. Crowe writes on ' Rainfall regime in the South-Eastern United States.' In former papers *J.*, **66**, 1940 and elsewhere) he has explored the problem of evolving a disciplined method of des-ibing the seasonal incidence of rainfall ; in the present paper he employs a straightforward and ective graphical method to throw emphasis on the seasons of most rapid increase or decrease of

rainfall rather than on the apparently 'driest' or 'wettest' months. Applying this to the south-eastern United States brings out what will to many appear as surprising regional variations in the average monthly incidence of rainfall ; and Dr. Crowe's pleasantly unpretentious treatment of the data not only proves effective ; he justifies the need for straightforward objective description as a necessary precursor to sound scientific analysis. Professor F. K. Hare of McGill University has contributed a most welcome essay on 'Climatic Classification.' This will provide for many an excellent and much needed critical introduction to the work of Köppen and Thornthwaite, taking into account the recent developments by the latter author; references too are abundant. In conclusion he calls attention to the need for more work on climatic classification which should take into account the findings of environmental physiology. This essay indeed provides persuasive arguments for a field of study to which in this country relatively little attention has been paid. Among the other essays there is little of direct meteorological concern, but Professor Buchanan's short essay on air transport although primarily dealing with economic aspects will interest those who fly. Mention may also be made of a superbly stimulating essay by Dr. G. H. T. Kimble, whose work will be known to many meteorologists, although this time he writes in a very different field.

G.M.

Recent additions to the Society's Library

British Rainfall 1948. Meteorological Office, M.O. 536. London, His Majesty's Stationery Office, 1950. Pp. v, 218, maps, tables.

H. E. Buckley, *Crystal Growth*. New York, John Wiley and Sons (Chapman and Hall), 1951. Pp. xv, 571, plates, diagrams.

S. Chapman, *The Earth's magnetism*, 2nd edition. London, Methuen, 1951. Pp. xi, 127, figs.

Climatological normals for Egypt. Ministry of War and Marine, Meteorological Department, Cairo, Egypt, 1950. Pp. vii, 157.

E. H. G. Dobby, *South East Asia*. London, University of London Press, 1950. Pp. 415, figs.

H. Israel (editor), *Das Gewitter*. Leipzig, Akademische Verlagsgesellschaft, 1950. Pp. ix, 249, plates, diagrams.

A. R. Low, *Normal elliptic functions ; a normalized form of Weierstrass's elliptic functions*. Toronto, University of Toronto Press, 1950. Pp. 27.

Meteoros. Buenos Aires, Servicio Meteorologico Nacional, No. 1, 1951. First number of a new meteorological periodical from Argentina.

Veröffentlichungen des Meteorologischen Instituts der Universität Berlin. Dietrich Reimer, 1936-1942. Complete series of the periodical.

K. Schutt, *Einführung in die Meteorologie auf Physikalischer Grundlage*. Berlin, Palmén-Verlag, 1950. Pp. 179, 86 figs.

Obituary

Mr. R. T. CORNISH, M.Sc.

Reginald Thomas Cornish, who had held Fellowship in this Society since 21 December 1949, ied on 20 March 1951 at the early age of 37. He was educated at Sir Walter St. John's School and ing's College, London, where he graduated with first-class honours in geography. During the 939-45 war he became a senior meteorological officer and was entrusted with a special and difficult sk demanding a high degree of discretion and discernment. In the performance of this important uty he displayed ability and resourcefulness of a high order. After the war Mr. Cornish received an ppointment as lecturer in geography at Birkbeck and University Colleges, London. His M.Sc. egree was awarded for a thesis on the regional geography of East Central Sweden – a subject on which e had the privilege of addressing the Geographical Section of the British Association in 1950.

E.L.H.

Professor V. F. K. BJERKNES, For. Mem. R. S., Hon. Mem. R. Met. S., Hon. Mem. R. S. E., Hon. LL.D. (St. Andrews).

Vilhelm Firman Koren Bjerknes, who died at Oslo on 9 April 1951 at the age of 89, was the last f the great pioneers who created the science of dynamical meteorology. He is best known as the under and inspirer of the Bergen school of ' frontal ' meteorology, but before that period he had ready made a number of original contributions of the greatest importance, including the circulation eorems, the construction of contour charts of isobaric surfaces, and the introduction of absolute nits of pressure.

He was born in Oslo in 1862, the son of a professor who specialised in hydrodynamics, and at e age of 20 he had already published original papers in hydrodynamics. Throughout his working fe he made many contributions to pure hydrodynamics in addition to his work in meteorology. In 893, the year of his marriage, he received a professorship at Stockholm, and there he developed his rculation theorems. He first considered the solenoids formed by the isobaric-isosteric surfaces, hich become significant when the horizontal temperature gradient is large, in relation to such roblems as the vertical circulation of sea-breezes. Some years afterwards he brought in the earth's otation. An important proposition included in these theorems is that when the area of a quasi-orizontal slab of air contracts, the circulation round its periphery is changed by the addition of yclonic rotation, with the reverse effect when the area expands. In co-operation with Sandström he roposed the millibar as the best unit of pressure and afterwards pressed for its general adoption, owerfully aided by Sir Napier Shaw. In 1907 he went to Oslo and some years later published a ook on *Dynamical meteorology and hydrography*. This included a discussion of the topography of obaric surfaces and it was shown that what is now called the geostrophic wind derived from the ight contours of an isobaric surface is independent of density, so that the same scale can be used r any surface.

From 1912 till 1917 he occupied a chair at Leipzig, but the war intervened and most of his udents were called up. He then accepted a special appointment at the Geophysical Institute, Bergen, ising from the difficult forecasting situation in Norway due to lack of information in war time. He d under him three young meteorologists of outstanding ability, his son J. Bjerknes (now a professor Los Angeles), T. Bergeron, and H. Solberg. The results of this combination are known to all eteorologists, and to some extent in a wider circle also. The most important discoveries were made the first few years, but the Bergen ideas continued to develop and expand their influence for over enty years. In 1920 Bjerknes and Helland Hansen invited Sir Napier Shaw to bring over a small itish delegation to discuss the new ideas. The other members were Dr. Richardson, Dr. Goldie, d the writer, and all remember the enthusiastic atmosphere of the Institute, the inspiring personality Prof. Bjerknes, and the overflowing hospitality displayed.

The idea that depressions originate as waves on a sloping surface of discontinuity appears to date om the Leipzig period, when Bjerknes was interested in the earlier work of Helmholtz. It is difficult assess the value of this conception. It has not led to any solution of the problem, but it proved a eful model for empirical investigations. In recent times the main emphasis has been on baroclinic nes, but Bjerknes's thought included these, and he introduced the terms barotropic and baroclinic o meteorology. He was primarily a mathematical physicist and was inspired by the vision of precise ethods of forecasting based on computations, as he emphasised in his inaugural address at Leipzig d on other occasions. This hope has not yet been fulfilled, and may never be, but he lived to see e world-wide influence of the ideas, techniques, and vocabulary of the school which he founded and

inspired, and of which his son was the leading exponent. Owing to the vast complexity of the atmosphere, synoptic meteorology has to proceed on empirical lines, but little progress can be made except on the basis of sound physical ideas. The earlier discoveries were clearly inspired by the ideas of V. Bjerknes, but as the subject developed further the connections between the empirical and theoretical aspects were inevitably much less close than they would be in a normal science.

He returned to Oslo in 1926 and lived there for the rest of his life though he resigned from his professorship in 1932. While there he completed his great work on *Physical hydrodynamics with applications to dynamical meteorology* with J. Bjerknes, Bergeron and Solberg as co-authors, which was published in 1933. This was followed in 1934 by a book on solar hydrodynamics. His later years were overshadowed by the second world war, with its negation of the ideals of international friendship and co-operation to which he was passionately attached, and by the tragic loss of his younger son, who had all the family brilliance, and who died fighting for the Allied cause.

He spoke English fluently and paid a number of short visits to this country, of which the latest was in 1946, when he was one of the three Norwegian delegates to the Newton Tercentary Celebrations. He was elected a foreign member of the Royal Society in 1933, and was awarded the Symons Medal of the Royal Meteorological Society in 1932.

C.K.M.D.

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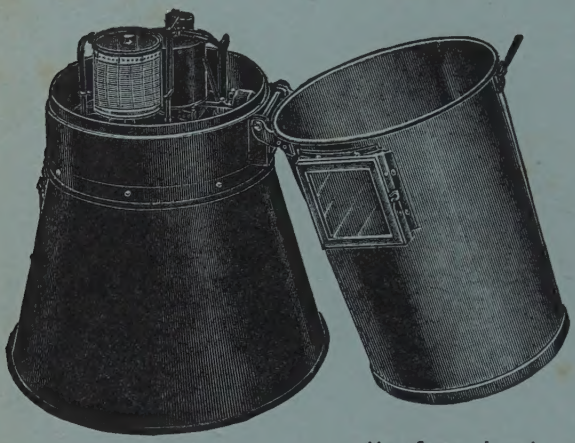

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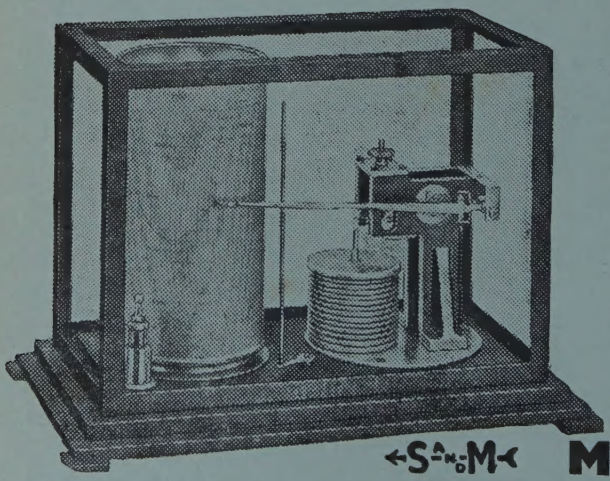

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